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THE MECHANISMS INVOLVED IN THE BEHAVIOR OF WHITE RATS ON THE INCLINED PLANE

From the Psychological Laboratories of Clark University

WALTER S. HUNTER

The present paper is a critical and constructive supplement to my earlier account of the behavior of white rats on the inclined plane (3). Since the publication of that paper many studies of the phenomenon have appeared by Crozier and his associates, following the early paper by Crozier and Pincus (5). One paper has appeared by Hovey (2) from the University of Oregon laboratory, and in addition Piéron (4) has published an article on the geotropic behavior of slugs. The papers by Hovey, Hunter, and Piéron, while differing in many particulars, all agree in opposing Crozier's description and explanation of the behavior concerned. It is not my intention to take up in detail all of the points of difference that are manifest in the investigations of the various authors nor to answer in a point-to-point fashion the criticisms which Crozier has made of the three experiments cited above (criticisms summarized particularly in 7, pp. 58-61). The research student who is interested in the description and explanation of the behavior of the rat on the inclined plane will in most cases be able to evaluate the arguments on the basis of the data already in print.

In my previous experimental study I confined myself to an examination of the possible correlation between the angle of elevation, α, of the plane and the angle, θ, at which the rat crept. In general the findings were as follows: (*a*) At α 25°-70° the rats crept at a great variety of θ values, most of which were negative to gravity, i.e., up the plane. The distributions of θ were extremely wide and tended to be skewed and bimodal. (*b*) Average and median values of θ increased with an increase in α. These measures of central tendency, however, were of doubtful value because of the character of the distributions. (*c*) It was impossible to verify Crozier's finding that when a rat stopped in its creeping, creeping was resumed either at the same angle θ, at its converse, or down the plane at the same angle. (*d*) No conclusions were drawn concerning the probable mechanisms involved in the rat's behavior.

In that experiment I used the method of recording the values of θ which is commonly employed, where the experimenter attempts to record the θ values on a sheet of paper while watching, or between periods of watching, the rat creep on the plane. No experiment has come to my attention where a highly accurate method of recording the behavior of the rat has been used. Such a method would involve either the *camera lucida* technique, familiar in maze studies, when high illuminations can be used or photography with ultra-violet light when work is done in darkness or in very low illumination. (The photographs could be time exposures, with luminous spots on the animal and on the plane.) Such accurate methods of recording behavior would undoubtedly also reveal a very wide range of θ values. The genetic constitution of my animals was probably not a source of variation in the results. The animals were, of course, not wholly homozygous, if such rats can be secured, but they were littermates of the long (more than 10 years) inbred Wistar experimental stock. My experiment was carried out under a weak diffused white light illumination the source of which was above and in front of the plane so that at α 45° the light was about perpendicular to the surface of the plane. Whatever effect such a light may have on behavior, no change in the behavior of these rats was found in the transition from the period before to the period after the eyes opened. Hovey (2, p. 551) reports no difference in behavior with daylight and red-light illumination. These findings are opposed to those from the Harvard laboratory where the effect has been more extensively studied. A satisfactory solution of the effect of non-directive light of various intensities upon the rat's behavior should be sought, but highly accurate methods of recording the rat's behavior must be used in such an investigation if the results are to be conclusive.

A thoroughly satisfactory answer to the question of the behavior mechanisms involved in the rat's creeping behavior on the inclined plane is rendered difficult, if not impossible, by the fact that at a given angle of elevation the rat does not creep at a definite angle θ, or even at a closely grouped series of θ values, but at a widely distributed series of values. This was shown in my paper (3) and in the work of Hovey (2). It is also clear in the ogive distributions given by Crozier and Pincus (6, p. 799) where, for example, the θ values for α 70° range from 48°-88°; and at α 45°, from 33°-88°. If a single definite value of θ were closely approximated for each value of α, then an analysis of the physics and physiology of the

situation should lead to some very illuminating conclusion, and the detailed mathematical treatment of the data would be better justified than it is at present. The importance of this point will, I trust, be clear as our discussion proceeds.

Since Crozier has vigorously defended the view that the behavior of the rat on the inclined plane is to be explained on the basis of muscle-tensions in the symmetrical legs of the young rat, thus assimilating the behavior to Loeb's doctrine of tropisms, we shall begin by presenting and analyzing Crozier's arguments. Later we shall outline our own conception of the mechanisms and factors involved.

In support of his explanation Crozier offers the following argument:

"We may consider, roughly and very crudely, that the orienting power is derived from the actions of levers on the opposite sides, and that the lever arm (x) on the 'down' side is shorter than that (y) on the 'up' side. Then the torque is responsible for turning upward. When the critical angle of orientation is exceeded, the locomotor action on the two sides of the body becomes equalized, so that if this value of θ is definitely exceeded (i.e., beyond a fluctuating zone, of increasing smallness as α is made larger), the rat is no more constrained than upon a horizontal surface, and is free to turn, should it chance to do so, until an equivalent θ is reached on the other side of the perpendicular. We have already described precisely this behavior. In the line of progression defined by θ, the locomotor effectiveness of the opposed limbs is just barely identical. This means that if we assume the axes of the legs to have mean positions perpendicular to that of the body, then ($x \cos \theta$—$y \cos \theta$) exactly balances the total downward pull of the animal's weight, where x and y are the 'lengths' of the legs as levers on the two sides. Hence,

$$(x—y) \cos \theta = G \sin \alpha$$

and

$$\frac{\cos \theta}{\sin \alpha} = \frac{G}{(x—y)}.$$

It was pointed out previously that the legs on the upward side are of course, more extended; hence, $y > x$; and if $(x—y)$ is constant for all magnitudes of θ, we have

$$—\cos \theta = K \sin \alpha \qquad (3).$$

"This relationship is in fact displayed, with unexpected exactness,

as shown in Figure 6,[1] and gives an independent means of checking the significance of the data summarized in Table 1.[2] The constancy of the quantity labelled $(x—y)$, derived from the applicability of (3), at once suggests that the difference between the work done by the limbs of the two sides of the body in lifting the animal's weight must be reduced to a constant fraction of the total before a stable orientation is attainable. This does not explain, of course, why the animal orients *upward* rather than downward, which may be determined by the inner ears; but it does explain why the amplitude of orientation attains its particular values as the inclination of the surface is varied, and in our opinion it gives an excellent illustration of the muscle-tension theory of orientation" (5, p. 265-267).

Before proceeding to a consideration of the formulae above presented, two other points must engage our attention. The first is the problem of the "critical angle" of orientation, and the second is the question of the relative lengths of the legs x and y.

If the rat's behavior in going up the inclined plane were due to symmetrical muscle-tensions, one would assume that all θ's would be approximately 90°, at which angle the legs on the two sides of the body would be equally excited. It is probable, however, that there is some threshold value of muscle-tension which may enter to complicate this otherwise simple situation. According to Crozier, when the rat is placed horizontally on the plane, it turns upward until a certain critical angle is attained at which the tensions on the two sides of the body are just above the threshold, or just at the threshold, and then creeps upward under such constraint. This point of view is present in the above quotation and is also illustrated in the following: ". . . when a rat, at first placed head downward, or with body axis horizontal, orients upward it does so until a certain quite definite angle has been reached, *and then progresses in a straight line*" (5, p. 260). "This clearly points to the limitation of geotropic orientation by a certain threshold determined through the distribution of the gravitational effect upon the two sides of the body" (5, p. 260). ". . . on the basic assumption that upwards orientation ceases with sensible equivalence of excitation on the two sides of the body, . . ." (7, p. 81). "On the assumption that orientation on a sloping surface is achieved when, within a threshold difference, the tension-

[1]This figure shows the relationship between cos θ and sin α, the former values decreasing as the latter increase.

[2]This table gives the average value of θ for each value of α.

excitations on the two sides of the body (legs) are the same, the angle of oriented progression (θ) can be taken as a direct measure of the total excitation" (7, p. 118). Between this value of θ and its converse the rat is no more constrained than on a horizontal plane because the differential tensions on the two sides of the body are subliminal.

The situation as so described is quite comparable to that found in any determination of the differential threshold. For example, if we have a light of a given intensity, there is a certain range above and below this intensity within which the subject's discriminative behavior is ruled by chance. If we are studying the limens for different points in the scale of intensity, it is the critical, limiting values above and below the standard light which will engross our attention. We will then determine that for light A the differential limen is x; for light B, y, etc. And such it would seem should be the procedure with the inclined plane problem, if Crozier's muscle-tension hypothesis were correct. The angles of elevation of the plane should be studied in relation to the critical or threshold angles of orientation and not to the average of all responses made. This follows because, according to Crozier, the rat's behavior between the critical value of θ on one side of the vertical and the same value on the other side of the vertical is unconstrained. To average all values of θ together is to make the average more representative of the many random orientations than of the (low) critical angles which may be present. From the statement that, between a given critical angle and its converse, the rat is no more constrained in its orientation than if upon a horizontal surface, it follows that such intermediate orientation angles cannot be functionally related to a, except insofar as their range is automatically limited by the increasing values of the critical angle. If we wish a value which will represent as nearly as possible *all* of the responses negative to gravity which the animal makes, we must use means, medians, or modes from the *whole* distribution. If, however, we are concerned with data throwing light on the muscle-tension threshold hypothesis, measures of central tendency should be drawn *only from the distribution of critical angles.*

In order to get some data on the value which the critical angle of θ would have at the various inclinations of the plane, I have proceeded as follows: The records of 4 young rats of the Wistar group (see 3, p. 303) were chosen. These 4 rats had responded almost entirely negatively to gravity. For each rat, 5 successive groups of

tests for each inclination of the plane were examined, and the lowest two values of θ were recorded. [The above quotation from Crozier (5, p. 260) would indicate that on his theory the critical value of θ would be the lowest θ for a given α.] If the lowest value of θ was zero, this value was not counted because there might be some possibility that the response had actually been slightly positive to gravity rather than of zero value or negative. There were thus secured 20 values of lowest θ for each rat, or 80 values in all at each value of α. I chose the lowest two values of θ in each group of tests rather than one in order to increase the dependability of the result. Table 1 shows the critical values of θ so determined.

TABLE 1

CRITICAL VALUES OF θ

α	Ave. θ	Median θ	Q
25	36	39	22
30	49	57	22
35	41	49	22
40	49	57	18
45	50	57	22
60	58	64	18
70	78	79	9

In general it can be said that the critical value of θ increases with an increase of α. However, the range of critical angles at each angle of inclination is tremendous and extends from 5°-85° at all inclinations save that of 70° where the range is from 45°-88°. Theoretically we may argue that *a* critical angle should, and does, appear in the behavior of the rat at each angle of elevation of the plane. Practically, no such fixed value occurs, and the rat is found to creep not only at a very great variety of angles θ but at a very great variety of lowest angles θ. It would therefore appear either that the theoretical analysis is faulty, or that a great variety of uncontrolled factors are influencing the behavior of the rat. We shall return to this topic later in the discussion.

Let us now turn to the question which remains to be considered before the mathematical analysis can be begun. When a rat creeps on the inclined plane, x designates the length of the leg on the downward side and y the length of the leg on the upward side. Crozier says that "the legs on the upward side are of course more extended;

hence, $y > x$; and if $(x—y)$ is constant for all magnitudes of θ, we have

$$—\cos \theta = K \sin \alpha'' \text{ (5, p. 266).}$$

Observation of the rat fails to support this statement. When the rat is creeping horizontally on the inclined plane with α above threshold value, the legs on the downward side are more extended than those on the upward side. This greater extension of the lower legs is readily observable as θ increases toward 90°. Although quantitative measures are difficult to secure, qualitative observation supports the following: y is always either equal to or less than x. $(x—y)$ is not constant for all angles of orientation, but decreases as θ increases, where α is constant. Where θ is constant, $(x—y)$ increases as α increases. $(x—y)$ is not a constant, but a variable; and it is always positive. Not only will a few moments observation of a rat on the inclined plane convince the reader of the essential correctness of the above qualitative comments, but everyday observation of animals climbing along a slope gives conclusions in harmony with the above. The leg, or legs, on the downward side are always more extended than those on the upper side. This is further illustrated in Hoagland's study of the behavior of the chick (1, p. 193) and in the drawing of a rat by Crozier and Pincus (7, p. 110) where the rear part of the rat seems still to be in the posture of normal creeping, but where the fore part of the animal is apparently turning to a new and higher angle of orientation.

If y is always less than x and if $(x—y)$ is a variable, then the equation $—\cos \theta = K \sin \alpha$ is incorrect and becomes $\cos \theta\ (V) = G \sin \alpha$, which is not the relationship found experimentally between the cosine of average θ and the sine of α. In the latter case it is found by all investigators that average θ increases with an increase in α, and it therefore follows that $\cos \theta$ decreases as $\sin \alpha$ increases. Crozier has used this trigonometric formulation in conjunction with his equation (3) to support the theory of muscle-tension. But if, as we have shown, equation (3) is wrong, and if the study of critical values of θ reveals the great range of such values, the theory of muscle-tension thresholds appears either wrong or much too simple.

If we were to develop an equation similar to Crozier's equation (3) utilizing the conception of the legs as levers with a mean position perpendicular to the line of progression, the procedure would be as follows: The pull of gravity, G, towards the center of the earth has one component, $G \sin \alpha$, along the surface of the plane (see

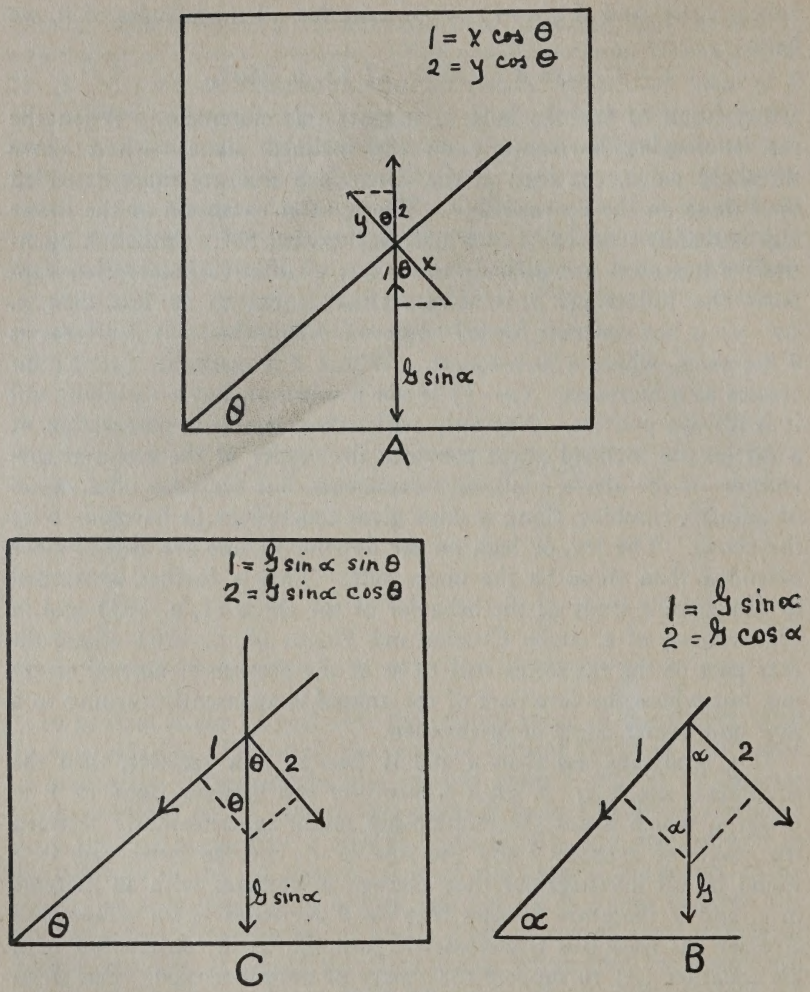

FIGURE 1

A. The negative forces, 1 and 2, exerted by the legs x and y against the positive forces of gravity, G sin α, along the slope of the plane. *B*. Resolution of G into the component along the plane, G sin α, and the component perpendicular to the plane, G cos α. *C*. The gravitational component along the line of progression, G sin α, sin θ, and the component perpendicular to this, G sin α cos θ.

Figure 1 *A* and *B*). This component may be termed positive. The pull of the upper leg, *y*, and the push of the lower leg, *x*, are opposed to gravity and hence may be called negative. The sum of these two negative forces plus the positive force of gravity along the plane, $G \sin \alpha$, is not equal to zero but to a negative quantity as shown by the fact that the rat creeps upward and not horizontally along the plane. We may then write the formula

$$(-pl \cdot y) \cos \theta + (-pu \cdot x) \cos \theta + G \sin \alpha = -K$$

where *pl* and *pu* are the negative pulls and pushes, *x* and *y* are the respective lever arms, and *K* is assumed constant as long as the rat creeps at a constant value of θ, without deviating up or down. Changing the signs and transposing, the above equation becomes

$$(pl \cdot y) \cos \theta + (pu \cdot x) \cos \theta = G \sin \alpha + K$$
$$\cos \theta \ (pl \cdot y + pu \cdot x) = G \sin \alpha + K$$

If the muscle-tension theory fails to give a clear explanation of the rat's behavior on the plane, can any alternative be suggested? Unfortunately the rat creeps at so many different values of θ for the same value of α that no single analysis of the physics and physiology of the situation can at present do justice to its complexity (see Hovey, 2, p. 557). We can, however, make a contribution to a better understanding of the behavior in question by analyzing certain aspects of the physical stimuli involved and by relating these to observable functional and structural peculiarities of the rat.

Let us first consider behavior. A young rat of the age used, about the last three days before the eyes open, if taken from the cage and placed upon a horizontal surface, will creep about on the plane making various angles θ with a given side of the plane. When the plane is inclined at 15° or more the random creeping of the rat takes on more and more an oriented character negative to gravity. The tilt of the plane and the consequent appearance of components of the gravitational pull serve essentially to give direction to the creeping movements although if the rat has stopped creeping an increase in the angle of inclination may serve to start the response again. At low values of α, the rat creeps at a wider variety of θ angles than it does as α becomes greater. Furthermore older rats who are stronger than the young ones creep at a greater variety of θ angles at all values of α than do the young animals. Since with low values of α the rat must expend relatively less energy in order

to cling to the plane than at higher values of α, we may say that, where the rat's strength is great relative either to the task to be performed or to the strength of other rats, the variation in θ will be relatively larger.

If a push or a pull is applied to a young rat that is standing on a horizontal plane, the animal reacts against the force applied. This is true whether the force is applied along the longitudinal axis or along the transverse axis of the animal. This reaction of the animal is a postural reflex. For our purposes we will distinguish between the response to a force exerted along the longitudinal axis and the response to one applied along the transverse axis. The first we shall call PR_1; and the latter, PR_2. No studies on the postural reflexes of the white rat have come to my attention; but I have attempted to secure some qualitative data as follows: With the rat on a horizontal plane, a string was attached about its body just behind the forelegs. When a transverse pull on the string was made, the rat not only actively opposed the pull but tended to turn at right angles to the force, i.e., tended to turn in such a way as to eliminate the transverse pull. If the string were pulled along the rat's longitudinal axis, the response was again against the pull plus, in many instances, a turning movement which threw the pull away from this axis. Obviously such tests were very crude. The force could not be applied with uniform intensity, nor could it be applied directly to the whole body. The tests did serve, however, to call attention to the two postural reflexes and to suggest that they be related to the types of stimulation which the rat receives on the inclined plane. On the inclined plane gravity supplies the longitudinal and transverse pulls which call forth the postural reflexes above described. Many complicating factors may enter in, particularly that of the relative strength of the animal and that of its tendency toward random creeping when removed from the nest, but the existence of the postural reflex mechanisms is beyond doubt. Furthermore the correlation of postural reflexes with the functions of the vestibular apparatus is more intimate than with muscle-tensions.

We are now ready to speak of the important components of the gravitational stimulus which come into play when the plane is inclined.[3] (See Figure 1, *B* and *C*.) The pull of gravity toward the

[3]I am indebted to Dr. J. W. Hedley of Regina, Canada, and a visitor in my animal behavior class at the University of Chicago, for outlining the desirability of the analysis of G beyond the component $G \sin \alpha$.

center of the earth does not vary with either α or θ, but it may be resolved into components which do so vary and which are related to the behavior of the rat. One of these components is along the slope of the plane and is $G \sin \alpha$. This is the component with which Crozier deals in all of his discussions. It could be treated, with relation to the orientation of the animal, as though it were a ray of light or a galvanic current, and the relation of $G \sin \alpha$ to the observed θ would readily suggest, following the tropistic tradition, the concept of muscle-tensions in symmetrical effectors. There are, however, three other components that are of great significance in the present problem. (*a*) The component perpendicular to the plane. This force, $G \cos \alpha$, is the one which holds the rat to the plane. (*b*) The component, $G \sin \alpha \sin \theta$, along the line of progression of the rat, which is the force that the rat is pulling directly against. (*c*) The third component is the one perpendicular to the line of progression and along the slope of the plane. It is the force which tends to roll the animal over, and is $G \sin \alpha \cos \theta$.

The component $G \cos \alpha$ increases with a decrease in α and vice versa. It is not affected by the behavior of the rat, but solely by the inclination of the plane. When $G \cos \alpha$ is large, the rat needs expend but little energy in order to maintain its footing. By raising and lowering the plane we are therefore essentially increasing and decreasing the muscular strength of the animal. It was seen above that the variations in angle θ were large for adult rats and for young rats at low values of α. We would conclude therefore that the behavior of the rat on the plane is partly a function of the difficulty of the task set by the experimenter as measured by $G \cos \alpha$.

The component $G \sin \alpha \cos \theta$, which varies directly with α and inversely with θ, is a transverse pull perpendicular to the rat's longitudinal axis and along the slope of the plane. It is a stimulus to which the rat will respond by active opposition and by turning to decrease the pull, i.e., by manifesting a postural reflex of the second type. As the rat makes this response, however, a complicating factor is encountered in the form of an increase in the other gravitational component.

This third component is $G \sin \alpha \sin \theta$, the pull along the animal's longitudinal axis. This component varies directly with α and θ. Therefore as the rat responds to the traverse pull by increasing θ and consequently decreasing that pull, it runs into an increase in the longitudinal component which arouses a postural reflex of the

first type. On the inclined plane it is impossible to decrease the transverse pull without increasing the longitudinal pull. If these two components were the sole determinants of the rat's orientation on the plane, that orientation should be at an angle θ where the two pulls were at a minimum. The actual angles of upward orientation, however, are widely distributed from 0-90° under the probable influence of such factors as (*a*) the random creeping of the rat when removed from the cage, (*b*) varying muscular strength as represented in $G \cos \alpha$, (*c*) momentary insecurity of footing which may lead to a marked increase of one component and to over-compensation, (*d*) fatigue in the execution of one of the postural reflexes. Thus in this latter case, the rat may be pulling primarily against the longitudinal component, at a high θ, the particular coordination of muscles so involved may become fatigued with the result that the rat turns to a smaller θ where another muscular coordination is involved but where the increase in the transverse component must now be overcome.

The behavior of the rat in going up the inclined plane, both with reference to the angle of orientation and to the negative direction of response, I regard as a result of the competition between PR_1 and PR_2, modified by factors of the above type. In executing such reflexes the rat so behaves that his equilibrium is best maintained. This he does irrespective of the consequences for muscle-tensions on the two sides of the body. (Thus a rat standing on a horizontal plane will push vigorously against a transverse force. Great asymmetry in muscle tension is apparent, but equilibrium is maintained.)

We have so far spoken as though the rat always went up, negative, on the inclined plane. This is not always the case, although the great majority of responses are negative and thus clearly in harmony with our hypothesis of the rôle of the postural reflexes. In addition to this factor making for negative responses is a structural peculiarity of the animal. As Hovey (2, p. 560) has pointed out, the location of the claws on the rat's feet is such that upward progression is easier than downward progression.

When the rat gets pointed downward on the plane at any considerable magnitude of θ, the normal postural reflexes which we have described would result in the rat backing up the plane. This response it does make very often for short distances. Since, however, backing is not the rat's usual mode of progression, the animal usually turns after some distance and progresses forwards at some angle θ.

If the rat does not back up the plane, in order to go upward it must turn the whole body against the pull of gravity. With high angles of α or θ the difficulty of making this rotation from head downward to head upward would seem sufficient to account for those few cases of downward progression which occur at the higher values of α. Where the α values are in the neighborhood of 25°-35°, where alone any considerable number of positive responses occur, the above factor plus the increased relative strength of the animal would seem sufficient to account for the behavior.

Conclusions

The analysis of the rat's behavior on the inclined plane leads me to reject Crozier's theory of muscle-tension in favor of the following: Both the negative direction of the creeping and the angle of orientation, θ, are due to (*a*) a combination of postural reflexes set up by certain of the components of the gravitational stimulus, (*b*) the relative strength of the animal, (*c*) the fact of normal forward progression with its correlate of claws on the anterior of the rat's feet, and (*d*) such factors as slipping and fatigue.

I have analyzed the gravitational pull effective on the rat when placed on an inclined plane into three components which are significant for behavior:

Component perpendicular to the plane	$G \cos \alpha$	(1)
Component along line of progression	$G \sin \alpha \sin \theta$	(2)
Component perpendicular to (2) and along the slope of the plane	$G \sin \alpha \cos \theta$	(3)

It has been shown that the effect of changes in (1) is essentially to increase or decrease the muscular strength of the rat relative to the work to be done. When $G \cos \alpha$ is large the animal wanders about on the plane, whereas when $G \cos \alpha$ is small, and the animal must expend much energy in order to cling to the plane, the variations in θ are small.

The ideal condition where at a given value of α the rat creeps at a constant value of θ would be accounted for by an equilibrium between, or an optimal combination of, the postural reflexes set up by the forces represented in equations (2) and (3). These postural reflexes are intimately connected with the functioning of the vestibular apparatus and, in their essentials, can be demonstrated without the use of the inclined plane. That the rat does not creep at the

above mentioned ideal (critical) value of θ for a given value of α, but at a large variety of θ's, indicates the presence of complicating factors other than the postural reflexes called forth by the gravitational pulls experimentally introduced.

Future experimentation faces the task of determining, with highly accurate recording methods, the precise nature of the factors making for variability in the interplay of the postural reflexes elicited on the inclined plane. The more that variability can be reduced the more justified will we be in the detailed mathematical analyses of the rat's behavior on the plane.

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II

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LES MÉCANISMES JOUANT UN RÔLE DANS LE COMPORTEMENT DES RATS BLANCS SUR LE PLAN INCLINÉ

(Résumé)

L'analyse du comportement du rat sur le plan incliné fait que l'auteur rejette la théorie de Crozier de la tension des muscles en faveur de la suivante: La direction négative du mouvement rampant et l'angle d'orien-

tation, θ, sont dus (a) à une combinaison de réflexes de posture causés par certains des composants du stimulus de gravitation, (b) à la force relative de l'animal, (c) au fait de la progression normale en avant avec la corrélation des griffes à l'antérieur des pieds du rat, et (d) à de tels facteurs que le glissement et la fatigue.

L'attrait de la gravitation sur le rat placé sur un plan incliné a été analysé en trois composants qui sont significatifs pour le comportement:

Composant perpendiculaire sur le plan	$G \cos \alpha$	(1)
Composant sur la ligne de progression	$G \sin \alpha \sin \theta$	(2)
Composant perpendiculaire sur (2) et sur la pente du plan	$G \sin \alpha \cos \theta$	(3)

On a montré que l'effet des changements en (1) est essentiellement celui d'augmenter ou de diminuer la force musculaire du rat relative au travail à faire. Quand $G \cos \alpha$ est grand l'animal va de long en large sur le plan, tandis que quand $G \cos \alpha$ est petit et il faut que l'animal emploie bien de l'énergie pour rester sur le plan, les variations en θ sont petites.

La condition idéale où à une valeur donnée de α le rat rampe à une valeur constante de θ serait expliquée par une équilibre entre les réflexes de posture causés par les forces représentées dans les équations (2) et (3), ou par la meilleure combinaison de celles-ci. Ces réflexes de posture ont un rapport étroit avec le fonctionnement de l'appareil vestibulaire et peuvent être montrés essentiellement sans l'emploi du plan incliné. Le fait que le rat ne rampe pas à la valeur idéale critique donnée ci-dessus de θ pour une valeur donnée de α, mais à une grande variété de θ's, indique la présence de facteurs compliquants autres que les réflexes de posture causés par les attraits de la gravitation expérimentalement introduits.

L'expérimentation future confronte la tâche de déterminer, avec des méthodes trés précises de notation, la nature précise des facteurs qui causent la variabilité dans le jeu entre eux-mêmes des réflexes de posture causés sur le plan incliné. Plus on peut réduire cette variabilité, plus on sera justifié dans les analyses mathématiques détaillées du comportement du rat sur le plan.

HUNTER

DIE MIT DEM VERHALTEN VON WEISSEN RATTEN AUF EINER SCHIEFEN EBENE VERBUNDENEN GETRIEBE

(Referat)

Die Analyse des Verhaltens der Ratte auf der schiefen Ebene veranlasst den Verfasser, Crozier's Theorie der Muskelspannung zu Günsten folgender Theorie abzuweisen: Sowohl die negative Richtung des Kriechens wie der Winkel der Orientierung (angle of orientation), θ, werden verursacht: (a) durch eine von gewissen Bestandteilen des Gravitationsreizes (gravitational stimulus) erzeugte Stellungsreflexenverbindung (combination of postural reflexes), (b) die relative Stärke des Tieres, (c) die Tatsache des normalen Fortschrittes vorwärts, in seinem Zusammenhang mit den Klauen auf der Vorderseite der Rattenfüsse, und (d) das Ausgleiten, die Ermüdung und dergleichen mehr.

Der Gravitationszug welche auf die Ratte wirkt wenn letztere auf die schiefe Ebene gesetzt wird, ist in drei Bestandteile auseinandergesetzt worden, welche für das Benehmen von Bedeutung sind, nämlich:

Der Bestandteil, der zu der Ebene senkrecht steht, G Kos α (1)
Der Bestandteil, der auf Fortschrittslinie (line of progression) liegt, G Sin α Sin θ (2)
Der Bestandteil, der zu (2) senkrecht, die Richtung der Ebene entlang, liegt G Sin α Kos θ (3)

Es ist erwiesen worden, dass die Wirkung von Änderungen bei (1) wesentlich ist, um die Stärke der Muskeln der Ratte im Verhältniss mit der zu leistenden Arbeit zu vergrössern oder zu vermindern. Wenn G Kos α gross ist, wandert das Tier auf der Ebene herum, wärend wenn G Kos α klein ist, und dass Tier viele Kraft verwenden muss, um sich auf der Ebene fest zu halten, die Abweichungen bei θ klein sind.

Der ideale Zustand, worin bei einem gegebenen Wert von α die Ratte mit einem konstanten Schnelligkeitswert des θ kriecht, würde sich erklären durch einen Gleichgewichtszustand oder eine optimale Verbindung (optimal combination) unter den Stellungsreflexen die durch die in den Gleichungen (2) und (3) vertretenen Kräfte erzeugt worden sind. Diese Stellungsreflexe sind mit der Tätigkeit des Vestibulärapparates eng verbunden, und können im Wesentlichen ohne Gebrauch der schiefen Ebene erwiesen werden. Dass die Ratte nicht mit der obenerwähnten idealen (kritischen) Schnelligkeit des θ für einen gegebenen Wert des α kriecht, sondern mit vielen verschiedenen θ, zeigt, dass auch andere komplizierende Elemente gegenwärtig sind, ausserhalb der durch die experimentell eingeführten Gravitationszüge hervorgerufenen Stellreflexe.

Zukünftige Versuche stehen der Aufgabe gegenüber, mit höchst genauen Methoden der Registrierung den spezifischen Karakter der Elemente festzustellen, welche Variabilität bei der gegenseitigen Wirkung unter den auf der schiefen Ebene hervorgerufenen Reflexen befördern. Je mehr man jene Variabilität vermindern kann, desto mehr wird man bei den detaillierten mathematischen Analysen des Verhaltens der Ratte auf der Ebene berechtigt sein.

HUNTER

AN INVESTIGATION OF BINOCULAR SUMMATION: II. THE PERIPHERY*[1]

From the Psychological Laboratories of Clark University

C. H. GRAHAM

I

The problem of binocular summation is a problem which involves some rather definite theoretical and practical implications. If there is a real summation it must be a central process. Hence, the demonstration of the phenomenon would provide experimenters with a very convenient method of attacking the problem of central function. No more convenient approach could be found than one by means of the eyes. In no other sense department can such accurate quantitative data be obtained. This attack is illustrated by Spencer's experiments on the verification of Heymans' law (18, 19) and by experiments on light-spot adaptation (5, 10).

The following report appears to show that there is no summation effect. The theoretical implications of this fact are of quite as much importance as if a summation effect were demonstrated. Much of the work in vision up to the present time has involved a comparison of adjacent retinal areas. König and Brodhun's work (14, pp. 135-139) on intensity discrimination is a classical example of this type of procedure. The work of Geldard (7) has shown that comparisons made under these conditions give entirely different results from those obtained when a binocular comparison is made, and Adrian and Matthews (2, 3) have demonstrated the presence of an interaction process during light stimulation. Further determinations concerned with the nature of the neural activity involved will clarify the facts of visual acuity and contribute largely to theories of stimulation. A method of attacking this problem is the method of binocular comparison. The question involved is whether or not a binocular comparison is a legitimate comparison, or whether it involves a central summation effect. If there is no summation effect it seems as though the method is a legitimate one.

*Accepted for publication by John Paul Nafe of the Editorial Board.

[1]The writer wishes to express his appreciation to Dr. Nafe for his help and interest in these experiments.

In a previous paper (8) the writer described an experiment which established the fact that there is no summation effect in foveal stimulation. In that paper there was a discussion of the more important literature on the problem. Particular attention was paid to the experiments of Piper (15) and Sherrington (17), and the negative results of Abney and Watson (1) were also mentioned. It was shown that the slight evidence for summation which Sherrington found might have been due to failure to control the size of the pupil. This point, however, is one which is extremely debatable, and the writer will only state that Sherrington's results are too equivocal to show any evidence for or against a summation effect.

Recently, reports of new experiments bearing on the problem have been presented. DeSilva and Bartley (6) have conducted an investigation in which the procedure was that of testing for a summation process by a comparison method. The apparatus was so constructed that light coming from one comparison field to one eye could be varied and measured. At the same time light from a standard field which always reached both eyes could be altered. The general procedure was the following. A rotating episcotister was interposed between one of the subject's eyes and the corresponding field after both fields had been equated for brightness. After the episcotister was introduced the two surfaces were again equated. It was found that the subject invariably reduced the intensity of the standard field in the second equation. The writers believe that these results demonstrate the presence of a measurable brightness change in binocular brightness when the illumination to one eye is altered.

There are certain criticisms which can be made of this work. (*a*) The use of an episcotister for varying intensity constitutes an unsatisfactory form of procedure. The assumption made is that Talbot's law is valid in the *given* situation. This is open to question. It might be that any apparent summation effect shown in this experiment may have been a function of intermittent exposure to light rather than to a decreased intensity of light. (*b*) Pupil area should be adequately controlled in an experiment of this type. DeSilva and Bartley have not provided this control. Their procedure allows the presence of several kinds of pupil phenomena which might influence the results (8). (*c*) Comparison surfaces, if they are to give good comparisons, should be contiguous (9). DeSilva and Bartley did not present an ideal comparison situation to their subjects. The visual pattern consisted of two discs of light separated by an ex-

panse of darkness. It is to be expected that discrimination would be poor under these conditions. To what extent these possible sources of error may have entered into DeSilva and Bartley's work is indeterminate. In general, however, it may be said that the experiment points out the possibility of greatly complicated reflex activity.

Angier and Cook (4) have reported an experiment in which they determined that the average monocular threshold after binocular light adaptation was, for three subjects, 50% higher than the binocular threshold—an evidence of foveal binocular summation. The summary of the experiment is so brief that a critical analysis of the procedure is impossible.

The experiment which is most relevant to our problem is that of Piper (15). Piper reports a summation effect for a field subtending a visual angle of 18°. His results show that with dark adaptation sensitivity is about twice as great with both eyes as with one eye. The writer, in the previous paper, presented some general criticisms of Piper's method, particularly those criticisms made by Hecht (11, 12). These criticisms have to do with the question of the initial state of light adaptation, the break from light adaptation to dark adaptation and especially the method of presenting the data. Piper reported his results in terms of *Empfindlichkeit.*

This measure is defined as a million times the reciprocal of the minimum intensity. Hecht (11) argues that a measure of this kind gives a distorted view of the course of dark adaptation. In recalculating Piper's data he took the reciprocals of the published values of *Empfindlichkeit.* The writer has done this with the data as given in Figure 1.

In this figure are presented two graphs showing the relative monocular and binocular thresholds of two of Piper's subjects. The semi-logarithmic form of graph is used for two reasons: (*a*) The range of minimum intensities is so great that a logarithmic plot is the only one that can give a picture of the process; and (*b*) the *log I* graph is the more legitimate one if we accept Hecht's hypothesis that the concentration of S is proportional to $-\log I$.

It is seen that for both subjects the binocular thresholds are consistently lower than the monocular after the first 10 minutes of dark adaptation. If one were to judge on the basis of these results alone, it would seem that the conclusion is inevitable that there is a summation effect. Before such a conclusion can be accepted, however, there are certain considerations to be noted. Piper reports the monoc-

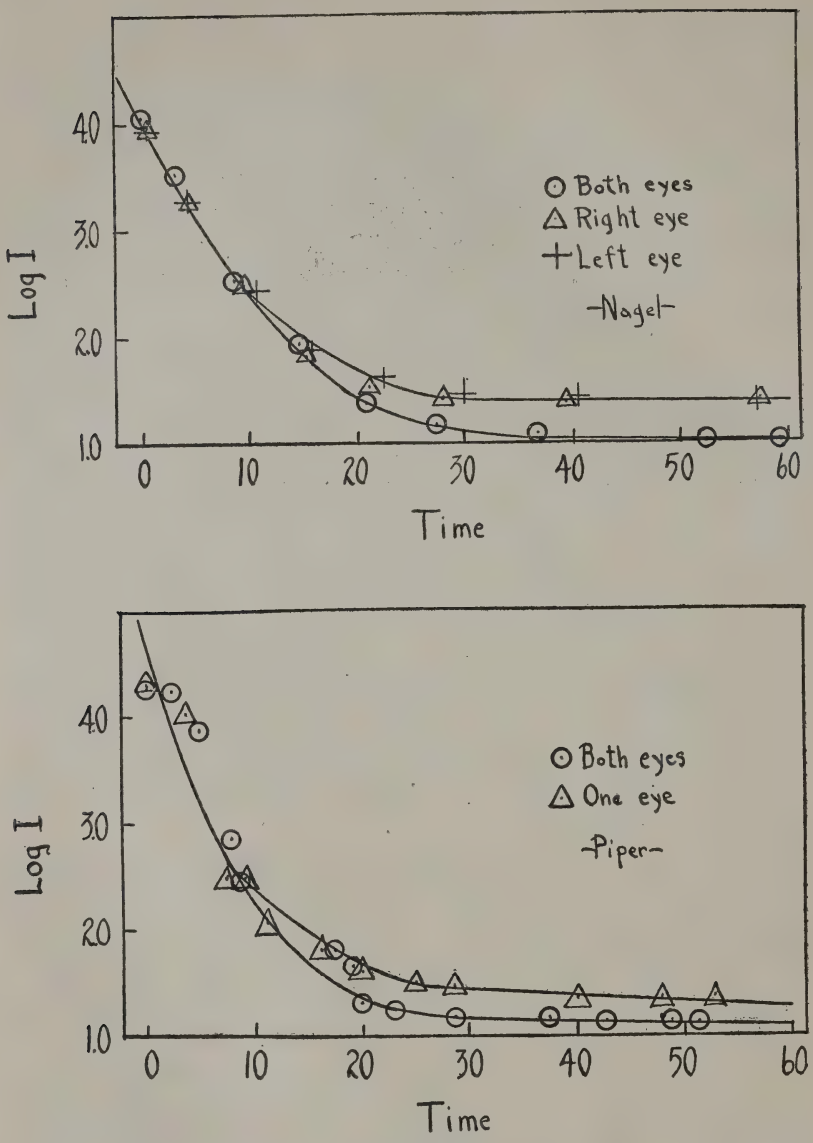

FIGURE 1
GRAPHS OF PIPER'S DATA RECALCULATED
(Explanation in text)

ular results of only two subjects. He does not mention how many were used in the second section of his work. In the determination of the binocular dark-adaptation process he used 18 subjects. It seems, then, that all his conclusions as to summation are based upon only two subjects. Furthermore, it seems, as the recalculated data are observed, that the differences between the binocular and monocular thresholds may fall within the experimental error. The course of the process was determined only once binocularly and once monocularly for each subject reported; hence, there is no way by which we can estimate the consistency of the subjects. However, individual differences from subject to subject are so obvious in the results that it is a fairly well-founded assumption that the results reported for the two subjects may entail a high experimental error which would wipe out any seeming summation effect. From this analysis of Piper's work the conclusion is drawn that any summation effect is highly questionable.

II

The first experiment reported here was done with a visual object (a disc of white light) the center of which was at an angle of 10° from the fovea. The red cross which was used as the threshold object in the experiment on the fovea was used as a fixation object. The apparatus used in this section for determining the dark-adaptation threshold with the peripheral stimulus is only a slight modification of the apparatus used for the fovea.

The fixation object is a red cross. This is made by cutting a cross-shaped area out of a piece of cardboard and placing it over a Wratten light filter, No. 70. The diameter of the cross is 24 mm. The filter with the cardboard forms the front of a small box in which is a small flashlight bulb connected with a dry-cell battery and a switch controlled by the experimenter. Behind the filter is a milk-glass plate of the same size as the filter. This part of the apparatus consists of a small, blackened, light-tight box, the only illumination from which is that through the cross-shaped opening. This small box is attached to a bracket which is screwed into the side of the long box of the previous experiment at a distance of about 1.5 cm from the end containing the peripheral stimulus object. The red cross and the circle are at the same level.

Instead of the Wratten light filter No. 70 of the previous experiment, a neutral-tint filter with a transmission coefficient of 1% is

substituted. Over this is placed a cardboard out of which a circle of 1.35-cm. diameter is cut. This serves as the peripheral stimulus object. Back of the filter is a milk-glass plate. Under these conditions the stimulus object is a disc of white light.

The light-adapting apparatus of the previous experiment is moved over a few centimeters so that the circular object is not on a line which bisects the line between the two observation openings. The apparatus is adjusted so that the red cross and the white disc are on the horopter. In this case light from the white circle stimulates "corresponding" points of the two retinas. The red cross is on a line which bisects the line between the two observation openings. The distance from the cross to these openings is 75 cm. The cross is to the left of the circle. The center of the circle is at an angle of 10° from the fovea.

The apparatus as now reported is controlled by the experimenter who is not in the dark room. One drawback of the apparatus is the fact that it does not provide a large enough range of intensities to determine the course of peripheral dark adaptation. In order to provide the necessary range of intensities a calibrated neutral tint wedge with a balancing wedge is used. The balancing wedge is placed before the disc and kept there in a holder. The wedge is placed in an adjustable holder and is so arranged that it can be moved back and forth across the front of the balancing wedge. In this way a very accurate measurement of intensity can be obtained. In practice, the wedge was put in a fixed position after the 6-minute reading and all the later adjustments of intensity were made by moving the 25-watt bulb in the long box. Calculations were made by the inverse square law.

Four subjects were used in this experiment. With all four of the subjects six complete series of determinations were made. The procedure in this part of the experiment was similar to that of the first section of the fovea. Three minutes were allowed for dark adaptation. This was followed by a 10-minute period of light adaptation. In the case of three of the subjects the first reading was made at 6 minutes in the dark. With these subjects an artificial pupil of 2-mm. diameter was used. The use of the artificial pupil made it impossible to get an early reading. With another subject determinations were made without the use of an artificial pupil. For him there was a reading after about 10 seconds in the dark. In this

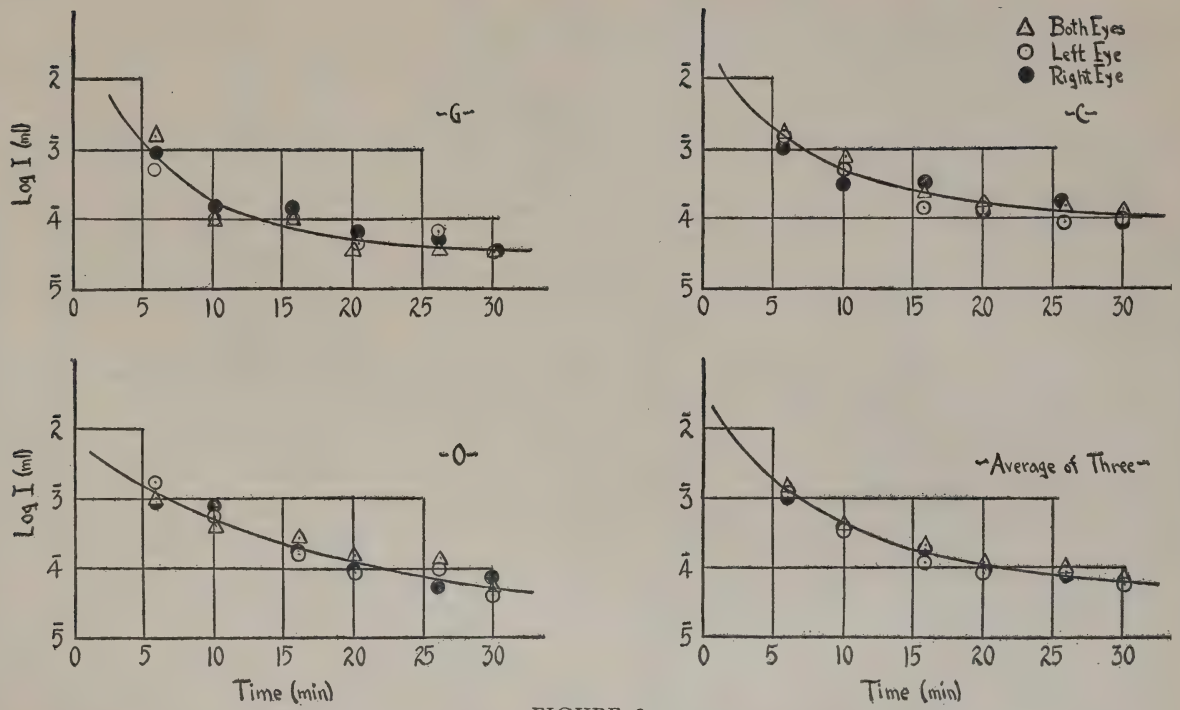

FIGURE 2

BINOCULAR AND MONOCULAR DARK-ADAPTATION THRESHOLDS FOR THREE SUBJECTS WITH ARTIFICIAL PUPILS

case the intensity was not changed. The subject simply reported, after light adaptation, when he saw the constant light.

In all cases determinations were made after 6, 10, 16, 20, 26, and 30 minutes. In the monocular series all the readings could be made with one light adaptation because of the fact that the eyes could be alternated. Under these conditions there was still an interval of 10 minutes between successive readings. In the binocular series two light adaptations were necessary. After the first light adaptation the order was: 6 minutes, 16 minutes, and 26 minutes. After the second light adaptation it was: 10 minutes, 20 minutes, and 30 minutes. In each observation period two series were run following two light-adaptation intervals.

III

The data obtained in this experiment are graphically presented in Figure 2. Here is presented a graph for each of the subjects with whom artificial pupils were used. Each point is the average of three determinations. In the case of the graph presenting the com-

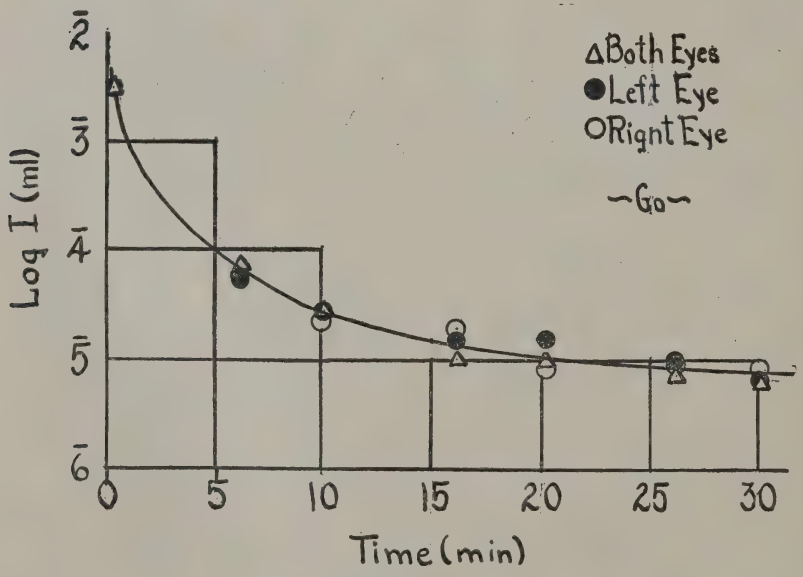

FIGURE 3*a*

BINOCULAR AND MONOCULAR DARK-ADAPTATION THRESHOLDS FOR ONE SUBJECT WITHOUT ARTIFICIAL PUPILS

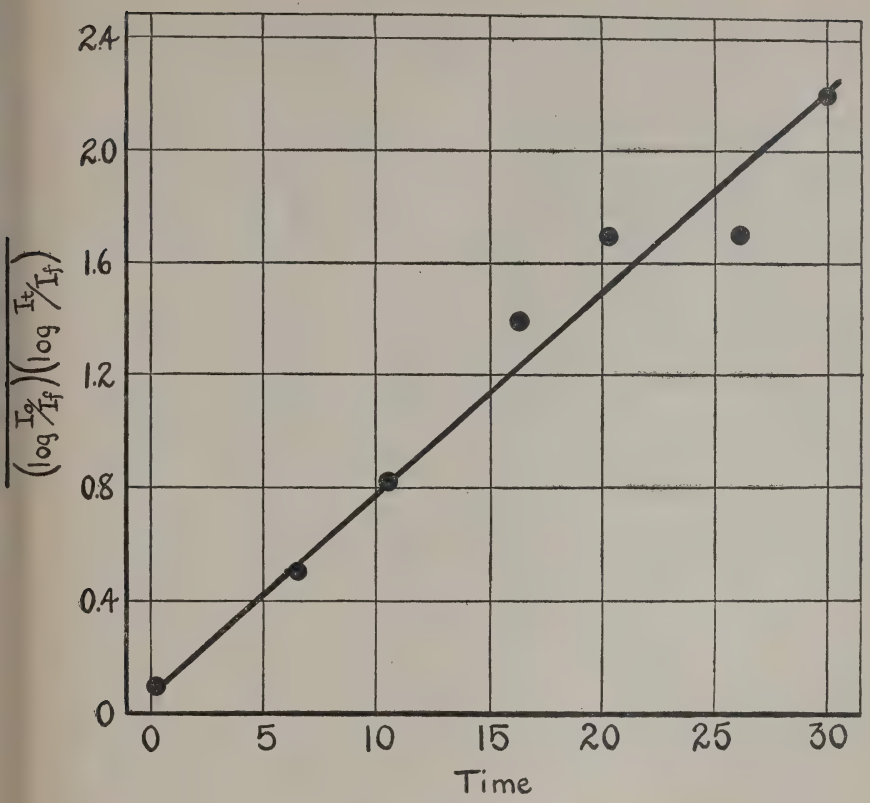

FIGURE 3*b*

RESULTS OF ONE SUBJECT PLOTTED TO SHOW HOW WELL THE DESCRIPTION OF THE DARK-ADAPTATION PROCESS AS A BIMOLECULAR REACTION FITS THE DATA OF THIS EXPERIMENT

posite results each point is the average of nine determinations. Table 1 gives the data from which these graphs were constructed. Figure 3*a* and Table 2 give the results of the subject with whom artificial pupils were not used.[2]

[2]Hecht (11, 12, 13) has demonstrated in a convincing manner that the dark-adaptation process is best described as a bimolecular reaction

$$k_2 = \frac{a - x}{tax} \qquad [1]$$

where k_2 is the velocity constant; t the time of dark adaptation; $a - x$ the

A survey of the data makes it plain that this experiment has not shown the existence of a summation effect. In all cases the monocular thresholds are consistently as low as the binocular thresholds. Nothing which may be interpreted as showing a summation effect is consistently present in the results of any subject. The tendency for the monocular thresholds to be lower than the binocular thresholds is just as prevalent as the tendency for the binocular thresholds to be lower than the monocular thresholds. The results of this experiment fail to show any evidence for a summation effect.

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concentration of photosensitive material already formed; and x the concentration still to be formed. It would be of interest to see how well the data of this experiment fit such a formulation.

The description of the dark-adaptation process as a bimolecular reaction involves two assumptions: (*a*) that the photochemical effect is proportional to the logarithm of the intensity of light, and (*b*) that the concentration of *S* (the sensitive substance formed in the dark) is proportional to the logarithm of the reciprocal of the intensity. If these assumptions are granted, Equation [1] may be changed so as to read in terms of light intensity.

Let I_0 be called the intensity threshold at the first moment of dark adaptation, I_t the intensity at any moment t, and I_f the final intensity at complete dark adaptation. "The concentrations of the sensitive material at these moments are then proportional to $-log\ I_0$, $-log\ I_t$, and $-log\ I_f$, respectively. The difference between $-log\ I_f$ and $-log\ I_0$ gives the total change in concentration of sensitive material during dark adaptation. This we may call a, which then becomes $a\ log\ I_0/I_f$. Similarly, the difference between $-log\ I_t$ and $-log\ I_0$ gives the change in concentration of sensitive substance already accomplished at the moment t. This is $a-x$ and becomes $a-x=log\ I_0/I_t$. Finally, the difference between $-log\ I_f$ and $-log\ I_t$ gives the concentration x of sensitive material still to be formed before dark adaptation is complete. Thus $x=log\ I_t/I_f$. Substituting these values in equation [1] we get

$$k_2=\frac{log\ I_0/I_t}{t\,(log\ I_0/I_f)\,(log\ I_t/I_f)} \qquad [10]$$

which may then be used for computing any set of experimental data" (13). It is plain from the equation that a plot of

$$\frac{log\ I_0/I_t}{(log\ I_0/I_f)\,(log\ I_t/I_f)}$$

against time should give a straight line. This procedure is followed with the data of Table 2, the results of the treatment being graphically presented in Figure 3*b*. The values of I_0 and I_f were not determined accurately, both values having been obtained by graphical methods. However, it is clear that the results are at least approximately in harmony with the bimolecular-reaction concept of the dark-adaptation process. In the results as presented I_0 was taken as 0.056 ml. and I_f as 0.000003 ml.

TABLE 1

THRESHOLD INTENSITIES FOR THREE SUBJECTS WITH ARTIFICIAL PUPILS

(Small visual object)

Subject	Both eyes Time	Both eyes I (ml)	Left eye Time	Left eye I (ml)	Right eye Time	Right eye I (ml)
G	6:10	0.00147	6:07	0.00053	6:08	0.00094
	10:10	.00010	10:05	.00010	10:08	.00015
	16:04	.00011	16:07	.00010	16:08	.00013
	20:05	.000036	20:04	.000039	20:06	.000067
	26:07	.000039	26:05	.000074	26:08	.000053
	30:07	.000034	30:04	.000035	30:04	.000039
C	6:07	.00177	6:05	.00154	6:10	.00111
	10:11	.00077	10:06	.00046	10:06	.00029
	16:05	.00024	16:06	.00014	16:11	.00031
	20:11	.00017	20:07	.00014	20:12	.00014
	26:08	.00016	26:07	.000095	26:13	.00017
	30:09	.00013	30:07	.00011	30:08	.000085
O	6:12	.00100	6:12	.00177	6:14	.00097
	10:05	.00044	10:08	.00058	10:25	.00094
	16:18	.00030	16:13	.00013	16:12	.00016
	20:03	.00015	20:13	.000085	20:22	.00010
	26:07	.00014	26:21	.00011	26:13	.000056
	30:02	.000056	30:13	.000040	30:22	.000068
Average	6:10	.00141	6:08	.00128	6:11	.00100
	10:09	.00043	10:06	.00038	10:13	.00046
	16:09	.00022	16:09	.00012	16:10	.00017
	20:06	.00012	20:08	.000088	20:17	.00010
	26:07	.00011	26:11	.000093	26:11	.000093
	30:06	.000073	30:08	.000058	30:11	.000064

TABLE 2

THRESHOLD INTENSITIES FOR ONE SUBJECT WITHOUT ARTIFICIAL PUPILS

(Small visual object)

Both eyes Time	Both eyes I (ml)	Left eye Time	Left eye I (ml)	Right eye Time	Right eye I (ml)
:17	0.0030	:10	0.0030	:13	0.0030
6:18	.000069	6:30	.000062	6:20	.000057
10:20	.000028	10:18	.000028	10:13	.000025
16:15	.000010	16:10	.000015	16:18	.000014
20:10	.000010	20:15	.000014	20:18	.0000083
26:17	.0000079	26:16	.000010	26:10	.0000083
30:12	.0000069	30:07	.0000079	30:05	.0000083

IV

The evidence discussed in the last section of this paper seems to point rather definitely to the fact that there is no summation process in the periphery. It must be remembered, however, that the results just discussed were obtained under conditions which are not directly comparable to those of Piper. In order to check thoroughly on the validity of Piper's results it is necessary to do the experiment under the stimulating conditions of Piper's experiment. It may be that a summation effect was shown in the latter's results because of the fact that a visual object of large area was used. Specifically, this brings up the question as to whether or not the reason that the results reported in the last section did not show an effect was because the area of the visual object was too small. It is likely that if there is a summation effect it varies with the number of fibers stimulated; or, in terms of the stimulating conditions, it varies with the area of the visual object.

In order to repeat the experiment under Piper's conditions a new type of apparatus was constructed. Figure 4 gives a side view of the set-up. All the parts of the apparatus are in the dark room. A large beaver-board screen (S_1) is placed upright on a stand (S_3). In the screen are the observation openings (OO) with a shield. This part of the apparatus is made so that the subject can be seated with his

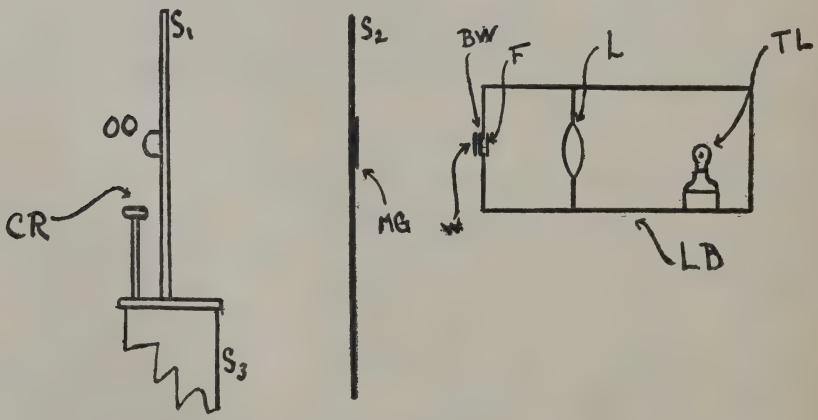

FIGURE 4
APPARATUS USED TO REPRODUCE PIPER'S STIMULATING CONDITIONS
Side view

chin on the chin-rest (*CR*) and fixate the stimulus surface of milk glass (*MG*). This surface is in the center of a large screen of beaver-board (S_2). The dimensions of this screen are 100 cm. by 100 cm. In the center is a square opening (10 cm. by 10 cm.). Behind this opening is placed the milk-glass plate (*MG*). The distance from the subject's eyes to this plate is 30 cm. This part of the apparatus reproduces the stimulating conditions of Piper's experiment.

The method of regulating the threshold intensities of illumination is different from that used by Piper. Piper used two methods: (*a*) In front of a plano-convex lens was placed a diaphragm, the area of which could be adjusted. The lens served to form a 10 cm. x 10 cm. image of an illuminated screen on a milk-glass plate which was observed by the subject from a distance of 30 cm. (*b*) Milk-glass plates were placed between the source of light and the illuminated screen. When the diaphragm was adjusted to its smallest area a set of milk-glass plates was put in and the diaphragm was opened again.[3] In this way a great range of intensities was available.

The apparatus for controlling illumination in this experiment is shown in Figure 4. A 25-watt concentrated-filament Mazda lamp (*TL*) is placed in a light-tight box (*LB*). In front of this lamp is a double convex lens (*L*) having a diameter of 12 cm. The rays of light are brought to a focus slightly in front of the balancing wedge (*BW*). The opening behind *BW* is a square of 1.5 cm by 1.5 cm. Behind this opening is a 1% neutral-tint filter (Wratten). Regulation of intensity is accomplished by means of the calibrated neutral-tint wedge (*W*) which is held in an adjustable holder and can be moved across the front of *BW*. The distance between the milk-glass plate (*MG*) and the wedge (*W*) is varied until the surface *MG* is uniformly illuminated. [The attempt was also made to have as little light as possible strike the area of the screen (S_2) outside of the stimulus object. Since there are two screens between the

[3]In a previous experiment (9), Nafe and the writer report results obtained under conditions in which a somewhat similar procedure was used, i.e., milk-glass plates were used for the regulation of intensities. While the method is accurate, it is very inconvenient. Since the dimming effect of a milk-glass plate depends more on its diffusing features than on its absorbing characteristics, it is impossible to get an absolute transmission coefficient. For example, the first plate cuts down the illumination striking the eye to about 5% of its former value, while the second plate cuts down the illumination to about 40%. Accurate results with this method can be obtained only when the transmission coefficient of each plate is measured in the situation in which it is to be used.

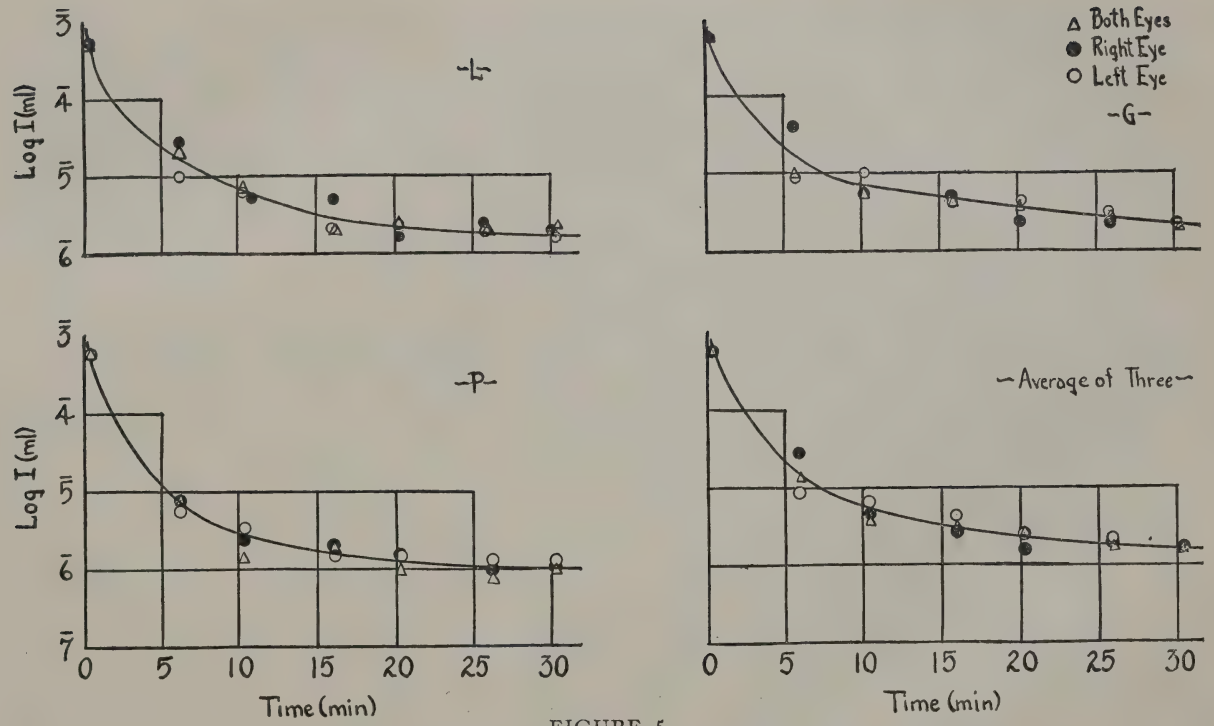

FIGURE 5

MONOCULAR AND BINOCULAR THRESHOLDS FOR THREE SUBJECTS UNDER THE STIMULATING CONDITIONS OF PIPER'S EXPERIMENT

subject and the light striking *MG* there is little chance for stray light to play a part. In the course of the experiment there was never a report of reflected light.] The brightness of illumination at *MG* is photometrically determined for a setting of the wedge (*W*). Readings were made by the experimenter by means of a small shielded flashlight, while the subject covered his eyes with black velvet during the 5 or 6 seconds necessary for a reading.

The light-adapting apparatus previously described is placed beside the subject. During the light-adaptation period the subject turned and looked at the illuminated screen. When a signal was given he turned his chair (always looking at the lighted area) and got ready to place his eyes at the observation openings. In all other respects the procedure of this experiment was identical with that of the previously reported experiment on the subject with whom artificial pupils were not used.

V

Three subjects were used in this experiment. Only one (*G*) had worked in the previous series. Figure 5 and Table 3 give the results obtained under these conditions. It is seen that there is no more evidence for a summation effect here than there was in the previous experiment. All of the results have failed to show the slightest trace of summation. It is hard to understand why Piper did get evidence for summation. A possible explanation, one which has not been mentioned before, although recognized throughout the experiment, is suggested by a fixation phenomenon noted during the progress of this investigation. The writer (the first subject) early found with himself a decided tendency to "squint" when the observations were monocular. An analysis of the data indicated that the results were affected by the procedure in such a manner that the monocular thresholds seemed for himself to be slightly higher than the binocular. The facts of the effect of accommodation on acuity would support such findings. Throughout the experiments instructions to the subject were rigid in stressing the necessity for uniform binocular and monocular fixation. In the series of experiments with artificial pupils reported in the previous paper a slide was used. In this case the subject did not know which eye was doing the observing. The results of this experiment support those of the total investigation.

TABLE 3

THRESHOLD INTENSITIES FOR THREE SUBJECTS UNDER THE STIMULATING CONDITIONS OF PIPER'S EXPERIMENT

Subject	Both eyes Time	I (ml)	Left eye Time	I (ml)	Right eye Time	I (ml)
G	:20	0.0006	:27	0.0006	:18	0.0006
	6:16	.00001	6:10	.000009	6:14	.00005
	10:11	.000006	10:10	.00001	10:09	.0000057
	16:12	.000005	16:11	.0000043	16:10	.0000048
	20:10	.0000043	20:10	.0000043	20:08	.0000024
	26:12	.0000026	26:08	.0000030	26:07	.0000023
	30:08	.0000019	30:09	.0000018	30:10	.0000023
L	:25	.0006	:26	.0006	:18	.0006
	6:17	.00002	6:33	.00001	6:23	.00003
	10:30	.0000038	10:23	.000007	10:40	.0000055
	16:20	.0000033	16:45	.0000021	16:30	.0000055
	20:23	.0000024	20:15	.0000024	20:20	.0000016
	26:18	.0000022	26:13	.0000021	26:18	.0000025
	30:30	.0000024	30:30	.0000016	30:10	.0000019
P	:20	.0006	:15	.0006	:13	.0006
	6:20	.000079	6:30	.0000055	6:05	.0000070
	10:20	.0000012	10:15	.0000035	10:15	.0000024
	16:30	.0000020	16:25	.0000016	16:20	.0000020
	20:20	.0000010	20:10	.0000016	20:25	.0000016
	26:20	.00000072	26:15	.0000013	26:14	.0000011
	30:15	.0000010	30:20	.0000013	30:05	.0000011
Average	:22	.0006	:23	.0006	:16	.0006
	6:18	.000013	6:24	.0000082	6:14	.000029
	10:20	.0000037	10:16	.0000068	10:21	.0000045
	16:21	.0000034	16:27	.0000027	16:20	.0000041
	20:18	.0000026	20:12	.0000028	20:18	.0000019
	26:17	.0000018	26:12	.0000021	26:13	.0000020
	30:18	.0000018	30:20	.0000016	30:08	.0000018

SUMMARY

The theoretical implications of the problem of binocular summation have been discussed. By means of an apparatus which has been described it was determined that there is no summation under conditions of peripheral retinal stimulation when the stimulated area is relatively small. An experiment in which the stimulating conditions of Piper's experiment were reproduced failed to show any evidence for a summation effect. In all cases the monocular thresholds are consistently as low as the binocular thresholds.

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UNE ENQUÊTE DE LA SOMMATION BINOCULAIRE: II. LA PÉRIPHÉRIE

(Résumé)

On a discuté les implications théoriques du problème de la sommation binoculaire. Au moyen d'un appareil qu'on a décrit on a déterminé qu'il ne se trouve pas de sommation dans des conditions de stimulation périphérique de la rétine quand l'aire stimulée est relativement petite. Une expérience où l'on a reproduit les conditions stimulantes de l'expérience de Piper n'a montré aucune évidence d'un effet de sommation. Dans tous les cas les seuils monoculaires sont constamment aussi peu élevés que les seuils binoculaires.

GRAHAM

EINE UNTERSUCHUNG ÜBER BINOKULÄRE SUMMIERUNG: II. DIE PERIPHERIE

(Referat)

Die theoretischen Verwicklungen des Problems der binokülären Summierung sind schon besprochen worden. Mittels eines schon beschriebenen Apparats wurde festgestellt, dass bei peripherischer Reizung der Netzhaut keine Summierung stattfindet wenn der gereizte Teil verhältnismässig klein ist. Ein versuch, worin die Reizverhältnisse aus Piper's Versuch wiederholt wurden lieferten keinen Beweis für eine Summierungswirkung. In allen Fällen waren die monokularen Schwellen übereinstimmend eben so niedrig wie die binokularen.

GRAHAM

DIFFERENTIAL RESPONSES TO CYCLIC PHASE VARIATIONS IN COMPOUND SOUNDS*

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The question as to whether a change in compound wave-form (maximum instantaneous pressure) due to an alteration in the phase relations of the partial frequency components (the root mean square analysis of the compound remaining constant) is discriminated as a change in tone quality attained paramount interest even during the earlier history of auditory theory preceding Helmholtz' monumental researches as assembled in his *Tonempfindungen* (1863).

Although this interest continued on the experimental side during the latter half of the nineteenth century, the problem has received little attention since 1909.

Helmholtz (10) consummated in 1877 a series of experiments concerning the influence of the relative phases of the partials on the "vowel quality" of a compound built up by sounds from ten to twenty electrically-driven tuning forks, and announced the much-quoted law: "The quality of a musical tone depends on the number and relative strength of its partial simple tones, and not on their differences of phase." He explicitly states, however, that "musical quality" means *vowel* quality. He did not, for example, consider a change in the *relative loudness* of the simple tone components as constituting a variation in "musical quality." Consequently, his "law" cannot be interpreted as meaning that phase is not of a *discriminable* order. Furthermore, the time consumed in resetting his apparatus for a phase change was too great to favor observation of minor differences in tone quality, if the latter is defined in terms of *discriminable differences.*

König (13, 14, 15) concluded from experiments with his wave siren that tone quality is affected by phase displacement of a harmonic. His conclusions are not convincing, much less decisive, since his wave siren does not produce changes in wave form corresponding precisely to changes in the curvature of the siren plates—an assumption upon which the tenability of his conclusions depends.

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Hermann (11), working with a siren, concluded in support of Helmholtz that phase displacements are irrelevant for hearing, and criticized König's work on the grounds that intensity variations were concomitants of the change in wave pattern on the siren disc.

Gray (8), as a result of his own experiments with tuning forks, concluded that "the single ear can distinguish no difference between two phases of a simple pure tone; neither can the ear distinguish a phase change in a complete harmony."

Emile ter Kuile (24), after a series of rather extensive experiments with tertiary harmonies from tuning forks, stated that he observed definite changes in the character of musical chords in which slow beats had been produced by mistuning one of the forks, and worked out a definite law for predicting the periods of these changes. This law has been stated and discussed in relation to the present problem (5). The "floating" or continuous change of phase used by ter Kuile for each successive cycle of the lower component (introduced by mistuning) is, however, *equivalent to the wave-form changes produced during one cyclic phase shift by means of the audio-phaser* (4).

Lindig (16, 17), using a "telephone-siren," concluded that the phases of the components influence quality of tone only through interference effects; that is, beats between identical frequencies. (*Einklänge*) introduced by overtones common to two systems in combination. By failing to record accurately the rates of phase changes together with the exact rates of loudness changes, Lindig did not establish his conclusion incontestably.

Wrightson (28, 29) insists that the problem has been conclusively solved in favor of Helmholtz' original assertion, whereas Meyer (20, 21) reasserts, on *purely theoretical* considerations, that under specified conditions phase alterations *may* produce audible differences in tone quality.

Lloyd and Agnew (18), using special alternating current generators in connection with telephone receivers, reported that phase differences of the components do not affect the quality of tone. They altered phase at too rapid a rate and by too large a phase angle for each shift to arrive at an exact statement. The discrepancies of Lindig's experiment were likewise present in that of Lloyd and Agnew.

Miller (22), after surveying the literature, states that while "systematic study of phase has not yet been made, no evidence has appeared which indicates that the phase relation of the partials has any

effect upon the quality of the tone." It should be added here: *no one* has demonstrated that phase *does not* influence quality of tone, since qualitative variations always accompanied phase alterations.

Watt (25) deprecates Meyer's theory (20, 21) and supports his own on the grounds that Meyer's theory admits discrimination of phase, whereas by his own theory such discrimination would be impossible.

Hartridge and Cosens (9), using a Helmholtz siren, concluded that "change of phase affects the quality of a musical chord if its constituent tones are accompanied by harmonics; but with pure tones, free from harmonics, change of phase does not audibly affect the quality of the mixed tone." They assign the changes observed to "beating among the partials" without showing (*a*) what rates of change they obtained, (*b*) what rates *could be present* due to "partial beats," (*c*) what rates *might be expected* in terms of the rate at which they altered the phase relations of their fundamentals, and (*d*) which tones varied in the manner specified. In the second case, that is, for pure tones, they used electrically-driven tuning forks. Here they had no control of phase; and so the second aspect of their conclusion is merely gratuitous, not experimentally validated.

Wilkinson (26), without having conducted any experimental work on the problem, is *final* on the point that phase *does not* affect auditory reaction.

It seems useful to reconsider the whole problem and devise a technique more adequate to the needs, since no one has yet obtained unambiguous results. The problem is no nearer to a solution than it was in the days of Helmholtz' initial experiment. Furthermore, no one has yet obtained *negative* results by actual *experiment,* but only through *inference as to the bases for the discriminations actually made.*

The apparent mutual consistency of most of these interpretations has been almost universally considered *prima facie* evidence for the utility of those theories concerning the mechanism of the cochlea which maintain that the receptor organ executes a resonance analysis of a compound air wave impinging on the tympanic membrane in a way analogous to that by which a harmonic analyzer differentiates the sinusoidal components of a compound periodic curve as given mathematically by Fourier's theorem.

The stimulus components *which become effective* as afferent impulses in the cochlear branch of the eighth cranial nerve, according

to practically all proponents of resonance theory, correspond in number and kind with those elements in compound sound waves which can produce a differential of response in a system of properly tuned physical resonators. Since the response of a resonator is determined by only the amplitude and frequency of the synchronized air wave which it reinforces, and is independent of the phase of the exciting wave, the assumed evidence that auditory discriminations are independent of the phase relations of the partials in a compound sound wave is supposed, consequently, to validate a general resonance theory of the receptor function. Such a sorites has been so widely adopted as to become almost veridical; and the *onus probandi,* therefore, rests with those who for various reasons question this theoretical position.

The major defects of previous experiments consist in the lack of quantitative analysis of the stimulus pattern in relation to the specific discriminations made during phase variation.

Experimental Procedure

The three major issues tested in this series of experiments are:

1. With a clang composed of two fundamentals, *L* and *H,* of approximately equal separate loudness, (*a*) What variations in clang quality occur during each cyclic variation of the phase of the *H*-component? (*b*) What partial components of the clang other than the two fundamentals are of discriminable magnitude? (*c*) Which components of the clang are discriminated as varying independently? (*d*) What are the relative sensation magnitudes and periods of these differential changes?

2. Can the steady phase condition be consistently discriminated from the cyclic phase retarding situation?

3. Maintaining a constant phase condition for the fundamentals, and varying only the phase relations between two partials of identical frequency (at the level of Lindig's *Einklänge*), are the same variations discriminated as in the case 1?

It is, apparently, a legitimate implication that if the discriminations of case 1 are significantly different from those of case 3, and case 2 is true, then the auditory relevance of "phase" is clearly demonstrated, *even though Einklänge are present in the compound sound.*

For the phase-retardation of the fundamental *H* the audio-phaser was used as a sound source. This apparatus has been described (4), but some details of the external circuit and auxiliary devices should

be mentioned. Figure 1 shows the experimenter's control and recording set-up.

It was found useful in preliminary instructions to use the loudspeaker (1, *A*) for the presentation of the tones to a group of *O*'s. By means of the attenuator (1, *F*) and the Wheatstone bridge, the intensity level of the currents could be varied logarithmically, and consequently, the sensation level of the sounds evaluated. The series connector (1, *G*) was used for permitting several *O*'s to observe simultaneously with separate headphones. A special ten-pen (1, *D*) polygraph with a 6-inch tape (1, *E*) was built to permit simultaneous records for eight *O*'s in addition to a seconds-timer and phase-rate indicator record. (Compare Figure 2, *infra*). The points of the ten pens lie on a line perpendicular to the direction of motion of the tape; and are, therefore, synchronized.

For individual subjects three types of headphones were used. All observations were made in a sound-insulated room, into which the conduit (1, *B*) opens. During simultaneous observation of several *O*'s Kellogg 65-A headphones were provided for each subject; but in making more crucial tests we used the Western Electric 555-W moving-coil receivers. These latter do not introduce audible harmonics into the output if the input level does not exceed a sensation

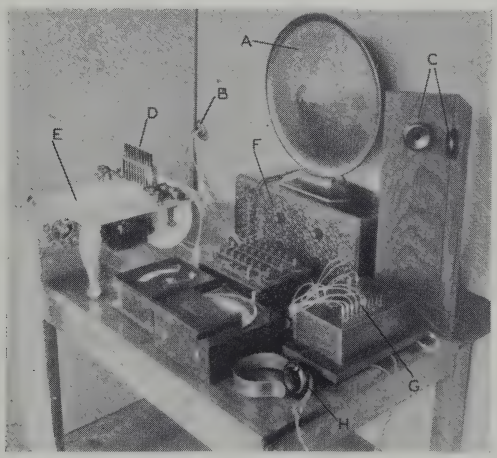

FIGURE 1

EXPERIMENTER'S CONTROL AND RECORDING APPARATUS IN CONJUNCTION WITH THE AUDIO-PHASER

level of 35 to 40 decibels. In other cases the Baldwin mica-diaphragm receivers were used. These introduce weaker and fewer harmonics than the Kellogg iron-diaphragm type.

The *O*'s were given preliminary instruction in the type of observation and report needed. Practice in discriminating frequency and intensity increments by means of tuning forks was introduced, so that *E* would be certain the *O*'s agreed on the qualities, *loudness* and *pitch*. This was accomplished by making *the increments intentionally large*.

Further practice was given in discriminating the removal of one or more weak partials from a clang produced by sounding five to ten tuning forks simultaneously. The purpose of this was twofold: to train the *O*'s in difficult clang analysis, and to inform *E* as to the abilities of the *O*'s to analyze clangs. Many *O*'s who failed to make the requisite discriminations in a preliminary series were eliminated.

The *O*'s were then rigorously practiced in a series of cases in which the audio-phaser was used as the source of sound. Every condition that was deemed relevant to the more difficult discriminations was presented for preliminary acquaintance. These series were not merely diagnostic of the *O*'s acoustic abilities but also furnished data that condition directly the conclusions to be derived from the final observations. This will become apparent after we have examined the nature of the series. Only the numbered statements were read to the *O*'s.

Series I

Case 1: The generated primaries (ratio 2:3; frequency level 960:1440, or slightly above or below this level; sensation level about 30 decibels) were presented simultaneously through the loud-speaker to a group of eight *O*'s.

1. What partials can you discriminate in this clang? Demonstrate them by selecting the proper forks on the table.

2. Observe this clang for five minutes and report if you discover any changing qualities, or if it appears to be a tonal compound in which the elements remain constant.

Case 2: Assistant in control room varies periodically the intensity of first one primary and then another. Other conditions the same as in Case 1.

3. Does the clang now appear constant or do you detect changing elements?

4. How would you characterize these changes?

5. Can you specify which partials vary in the manner you have indicated?

Case 3: Assistant in control room varies intensity of the two generated

primaries simultaneously, but in opposite directions; that is, primary 2 is augmented while primary 3 is diminished, etc. Other conditions the same as in Case 1.

6. Does the clang now appear to have changing elements

7. In what respects do the elements vary?

8. Which components of the clang vary?

9. Do they vary in the same manner at the same time? What is the temporal condition of their respective variations?

Case 4: Case 1 repeated.

These four cases were repeated five times in the following sequences: 1, 2, 3, 4; 2, 1, 4, 3; 4, 1, 3, 2; 1, 4, 2, 3; 3, 2, 1, 4. This was done in order to ascertain if the procedure had established a "set," giving *O* a tendency to report changes when none were being effected physically; and to ascertain if any fluctuation of attention were present here, leading *O*'s to report "loudness" or "pitch" surges when the tone-phaser output was operating in a steady state. If such tendencies were present they would necessarily have to be considered in the *O*'s reports later.

Case 5: Tone 3, delivered by the phase-changing inductor, was presented to each *O* through the loud-speaker, while the phase coil remained stationary. Five minutes for observation.

10. Can you discriminate now more than one partial?

11. Does the most conspicuous partial remain constant in pitch and loudness, or do you detect any changing features?

12. Identify the major partial and any others you can discriminate by selecting the proper tuning forks on the table.

13. Do any of the weaker partials appear to vary in any manner whatever? Do you detect any beats or variations in loudness?

Case 6: Same as Case 5, only the phase coil was rotated continuously throughout the observation period of five minutes. Questions of Case 5 repeated.

Case 7: Same as Case 6, with the exception that the phase coil was alternately *stationary* and *moving* for durations of one minute each for six minutes.

Cases 5, 6, and 7 repeated three times.

Case 8: Tone 2, delivered by a coil that was stationary at all times, was presented through the loud speaker. Questions of Case 5 were repeated. Case 8 was repeated three times.

Case 9: Tones 2 and 3 presented simultaneously through the loud-speaker for three periods of five minutes each, while the phase changing coil *rotated* continuously. The time duration for a cyclic phase shift was varied between 1 and 8 seconds. The optimum rate selected at this time was 4 seconds.

14. What partials do you discriminate in this clang?

15. Does the clang remain constant in all respects, or do you detect changing elements?

16. Which partials change? In what respects? In pitch or loudness or both? Do any new partials appear to enter the clang at one time or another, and disappear at times? Do these varying partials appear to change in the same way at the same time, or in different ways at the same time? Just what appears to be the temporal condition of the change in one partial with respect to changes in each of the others?

Series II

The nine cases of Series I were repeated in the same manner, with the exception that the tones were presented to *O*'s by means of double headphones, instead of through the amplifying unit and loud-speaker.

Series III

The nine cases of Series I were repeated in the same manner, with the exception that both generated primaries were presented in combination to *O*'s by means of single headphones.

Series IV

The nine cases of Series I were repeated in the same manner, with the exception that when both generated primaries were presented simultaneously, one was presented in a single headphone, the other was presented through a different single headphone *to the same ear.* These headphones were electrically independent of each other, and were mounted as shown in Figure 1, *C.* Individual observations replaced groups in this series.

Series V

This series was designed to test the discriminations made when the phase between the fundamentals remained constant and only the phase between harmonies of identical frequency at the *ratio* level of 3x2, that is, 6, varied. For this an exploring frequency from an oscillator would have been ideal; but in the absence of this, tuning forks were used. With the fundamentals 256 and 384, in combination with two forks at the frequency level 768, one of which was mistuned by varying amounts, the desired condition was approximated. The defect of this method is that the tuning fork tones do not continue sufficiently long for the *O*'s to make differential analyses. The relevant questions from Cases 1-9, Series I, were used in this series also.

Since in Series I-IV, Case 9, all *O*'s agreed that each discriminable partial varied periodically in some respect (pitch, loudness, quality, etc.), but apparently at different times, they were given an opportunity to demonstrate objectively the regularity and relative instants of these changes, as well as the frequency of the periodic

changes with respect to the rate of the cyclic phase shifts. Each *O* was provided with a silent contact key which operated a separate polygraph pen.

Instructions read: Since you have reported that each detectable partial in the clang appears to vary either in pitch or loudness periodically, but at different times relatively, you are now to indicate by pressing the key how and when a *particular* partial changes. Rest the index finger of your right hand lightly on the finger knob of this key at all times. When the partial you have been instructed to observe in particular appears to arrive at its *highest* "pitch" value, or its *maximum* "loudness," quickly press the knob down all the way, and hold it there until the *same* partial appears to arrive at its *minimum* "loudness" or its *lowest* "pitch." At that instant quickly release the pressure on the knob, permitting it to rise to its uppermost position. Leave the knob in this position until the same partial has arrived at its *maximum* "loudness," or *highest* "pitch" again, then repeat the preceding movements. Do this continuously as long as you hear the clang in the headphones.

Each *O* was given three five-minute practice periods only for each partial that *O* could easily discriminate and follow. This practice completed the preliminary training series. The *O*'s were then divided into four groups, with four *O*'s in each group, for special simultaneous performance. Each group of four practiced reacting simultaneously to changes in a single partial; the four partials used are in the ratio 1:2:3:4. The tone 6 was neglected in the recording, because it was so weak that only a few of the *O*'s could discriminate it at all, and even these were doubtful of it most of the time.

Each *O* was provided with a set of headphones, a silent contact key, and a chair with an arm on which to rest the hand in use. The sound room, in which each group practiced separately, was pitch-dark during the recording, so that *O*'s could not obtain visual cues by the others' reactions nor hear each other making contact. With this contact key, only a faint click can be heard when the key is operated one-half inch from the opening of the external auditory meatus.

Records in which the reactions of eight *O*'s were registered parallel to each other were occasionally taken on the polygraph during these practice periods. When it appeared that each group was fairly consistent within itself in discriminating the relative times and directions of the "variations," the final records were made, in which we refer to the four groups as follows:

Group 1: reacting to variations in the difference tone 1.

Group 2: reacting to variations in the lower generated primary tone 2.

Group 3: reacting to the higher generated primary tone 3.

Group 4: reacting to the only *audible* harmonic, tone 4.

From the final grouped records we wanted to measure:

1. The *rate* at which the changes occurred with reference to the rate of cyclic phase change. The phase contact operated a pen on the polygraph, making one displaced segment of the line for each cyclic change of phase. By comparing on the same record tape the time intervals of displacements due to the phase breaker with those due to the *O*'s independent reactions, this relation could be determined accurately and readily.

2. The relative times during which these periodic variations occur. That is, when the difference tone 1 increases, does the primary tone 2 increase or decrease, etc.? This was accomplished by various pairings of groups recording simultaneously. The groups were paired in successive recording periods as follows: 1 and 2; 1 and 3; 1 and 4; 2 and 3; 2 and 4.

Descriptive Results

In the preliminary experiments with tuning forks, we selected the 16 *O*'s out of 40 who revealed the best skill in discriminating small changes in pitch and loudness, counting beats in mistuned binary and tertiary harmonies, identifying difference- and overtones. The resulting group served ably throughout the remainder of the experiment. Two of these *O*'s had the alleged "absolute" pitch, four others were trained musicians, three were psychologists, and the remainder were students in the elementary psychology course. The latter had varying degrees of training in music, but were practically naïve as regards experimental acoustics.

Series I

Case 1. Eighteen *O*'s identified the primaries 2 and 3, the difference tone 1, and the first overtone 4 of the primary 2. Eight who could not identify 4 were eliminated. No one reported variations in the elements of the compound.

Case 2. In the cases where the intensity increment was large, each *O* identified the discrimination change as *loudness* (or some called it "intensity"), and each identified correctly *which* component was altered. However, when the intensity increment was *small,*

about 60% of the *O*'s called the change "pitch," and about 40% called it "loudness"; moreover, in this case, there was more difficulty in identifying which component was altered. The variations were *always* referred to either the fundamental 2 or 3, and never to the components 1 and 4. Changes attributed to one component, however, did not apparently induce the opposite variation in another component.

Case 3: This pattern of changes was confusing to each *O* at first. After several repetitions of the conditions, using both large and small increments, a fair degree of consistency resulted. But, as in Case 2, there was uniform characterization of the change as *loudness* only when the increments were *large*. For minimal changes, there was again the division of report between "pitch" and "loudness." Each *O*, however, discriminated both fundamentals as varying in an *opposite* manner, however the change was described verbally.

Cases 5, 6, 7, and 8. The object of this set of observations was to ascertain if the *O*'s would report variations in either fundamental under a steady condition, or in the fundamental 3, when the phase-changing disc rotated. When either fundamental, presented alone, was at a sensation level of about 25 to 35 decibels, no variations of short duration were discriminated; however, several *O*'s mentioned a gradual "fading" and return, lasting 15 to 20 seconds. Perhaps this was due to a fatigue effect or a postural change. At lower intensity levels, however, the fading was more marked; and so it was probably *not* a fatigue effect in either case. There were no variations which differentiated the condition of the phase coil rotating from the steady state for either fundamental.

Case 9. During the initial presentation of this condition (both fundamentals, with continuous phase-change) each *O* reported "many," "several," "a complex mixture," etc., of "pitch," "loudness," "quality," "pattern," etc., variations occurring simultaneously in the clang. Some *O*'s attended at first to the complex general pattern of change, being unable to identify specific variations. After several repetitions of this condition, each *O* learned to respond selectively to particular changes, although they did not agree on the verbal characterization of the change. Each *O* counted the same number of variations in tone 2, for example, but some insisted the variation was "pitch," others that it was "loudness" or "intensity." Several described figure-ground relations, that is, they said one component stood out from the background of the harmony periodically,

then another would appear, and the former recede. It was obvious from the protocols obtained in a series of these cases that whatever the reaction processes may be that result from this type of stimulus variant, they are not of sufficient magnitude to remove ambiguity. This set of observations clearly suggests that there must be a "threshold of difference" in discriminations before uniform verbal report can be secured. Minimal changes in intensity, frequency, and phase, although *discriminated,* are apparently *not differentiated* neurologically.

The *O*'s demonstrated by counting that the difference tone 1, and the two primaries 2 and 3, varied at the same rate but at different instants; and that the overtone 4, though very weak, could be discriminated as varying twice as rapidly as either of the other three.

Series II, III, and IV

No essentially different protocols were obtained in these series of observations. For most *O*'s, the difference tone 1 was much less conspicuous in Series II and III, and for some it was totally absent in Series IV. Those who identified this tone in Series IV reported that it did not vary in any manner. It should be noted that in Series IV the difference tone 1 was "subjective"—that is, there was not a component of the air wave at this frequency level; whereas in Series I, II, and III it was "objective"—there was a constituent in the air wave at that frequency level (cf. 17, 19). Only three *O*'s reported the overtone 4 in Series IV, where it was identified as a *very weak* variation occurring twice as rapidly as the alternate changes in the fundamentals 2 and 3. The phase-changing situation is ideal for showing up harmonics in the compound, for if any are present above threshold value, their periodic summation is readily detected. Series IV, consequently, presented a case of practically pure tones (psychologically).

Series V

Descriptions of the variations observed in Series V were clearly different from those of the cases where the phase between both fundamentals and partials of identical frequency occurred simultaneously. With the steady phase state for the fundamentals, and with the mistuned unison (768 cycles), the only discriminable variation was the "beat"—periodic variation in loudness—of this high-pitched partial. At first, the fundamentals were reported to carry slow beats; and so, working on the assumption that they were slightly mistuned rela-

tive to each other, we added wax to the prongs of the 384-cycle fork until when 256 and 384 alone were bowed simultaneously no beats could be discriminated. This was a painstaking task, and only four *O*'s were used for observation.

With this perfect relative tuning of the fundamentals, the addition of the "beating" pair of mistuned partials at the level 768 did not for any of these four *O*'s induce a loudness variation in either of the two fundamentals.

It was apparent from this special test that a mistuned binary *alone,* without audible mistuned unison harmonics, presents a variation in the relative loudness of the two fundamentals. From this, the basis for Smith's beat (6, 23, pp. 56 ff.) as a phenomenon distinct from the Helmholtzian theory of beats is clearly demonstrated, since loudness variations are discriminated when harmonic interference is *absent,* and the primaries are slightly mistuned from the perfect diatonic interval. In other words, *both theories are true.* Thus, a heretofore assumed, but *unverified,* basis for discriminating minimal loudness variations in mistuned pure binary harmonies is demonstrated. The relation between this principle and the discrimination of the cyclic phase shift is described in another article (5), where it is shown that the two conditions, on the stimulus side, are equivalent.

Results from Records

Many problems of psychological interest were presented in those instances where individual *O*'s were attempting to react overtly to a *specific* change occurring simultaneously with at least three other *specific* changes of a different order, making heterogeneous "total patterns" of a confusing character if attended to "as a whole." In the absence of the ability on the part of the *O*'s to selectively respond to *discrete* segments of the complex total stimulus pattern, the records we finally obtained would not have been made. Nevertheless, it was apparent throughout the preliminaries that *O*'s were *learning* to attend selectively to partial aspects of the total change of pattern. Many yards of records showed in the initial stages *no simultaneity* of response by *O*'s instructed to attend to the same variations. *Persistence* of discrimination was equally irregular. The *O*'s reported difficulty in following changes in one partial for any longer than a half-dozen cyclic phase shifts. Following this, they would react to first one, then another, of the partials; and finally return to the one followed originally.

Another difficulty encountered is that this tendency to shift attention from one varying component to another did not occur simultaneously for different *O*'s; consequently, there would be no objective criterion for differentiating the relative *directions* of change in the various components so long as any four *O*'s, presumably reacting to a specified component, showed no high degree of (*a*) consistency with each other and (*b*) difference from another group, consistent within itself, reacting to another component. Even in the initial stages, however, all the *O*'s reacted with only *two periods*: one equal to that of *one* cyclic phase shift; the other for *half* that duration. *O*'s reported that the fundamentals *H* and *L* and the difference tone *H-L* changed at the *same rate* but in *different directions,* these cyclic changes lasting for the same duration as one cyclic phase shift; but the octave overtone 2*L* of *L* changed twice as rapidly as any one of the other three components (cf. Figure 2). Group 4 from the beginning showed no tendency to react to the periods of *H, L,* and *H-L,* and gained internal consistency with respect to *direction* much sooner than the other groups. After a total of one and one-half hours' practice (18 five-minute periods), Groups 1, 2, and 3 began to show a fair degree of internal consistency and inter-group differentiation. Only for short periods of time, however, would the *O*'s react persistently enough to make usable records. While one or two *O*'s were reacting, one or two others would not be; when the latter two would start again, the former two might

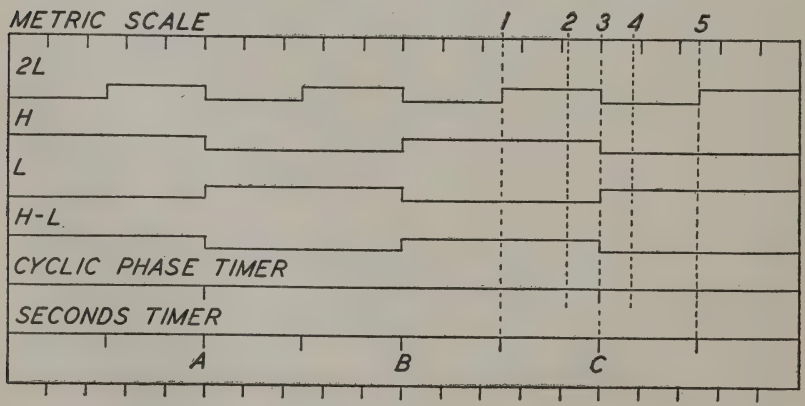

FIGURE 2

SYNTHETIC REPRESENTATION OF DISCRIMINATIONS MADE BY SIXTEEN OBSERVERS

cease, etc. Finally, we obtained six to ten records for each pairing of groups, in which all eight *O*'s were reacting persistently for 10 (minimum) to 18 (maximum) cyclic phase variations of the *H*-component.

From these records we derived a synthetic graph (Figure 2) showing the relative *periods* and *directions* of the four components of the clang as discriminated by 16 *O*'s in relation to the rate of cyclic phase shift for the *H*-component.

The downward excursions of the cyclic phase timer line mark the duration of one cyclic phase shift. This was for four seconds as indicated on the seconds timer line, and for ten units as indicated on the metric scale line. The record lines for *H-L, L, H,* and *2L* are for Groups 1, 2, 3, and 4, respectively. *Downward* excursions of these pens indicate the *interpreted* instants at which the *O*'s discriminated a component at its *maximum* loudness; the *upward* excursions indicate the instants of minimum loudness. In the *actual* records, the range (maximum) deviations of the *O*'s reactions in either direction from the *interpreted* point of change are indicated by the boundary dashlines *2* and *4*. This range was computed as $\pm 8.9\%$ of the distance *A-C,* which marks one cyclic phase shift. During this period, each *O* of Groups 1, 2, and 3 reacted *twice,* so the range of error would be $\pm 17.8\%$ of this time duration. For the same period each *O* of Group 4 reacted four times, making the maximum error $\pm 35.6\%$ of the time intervals for successive reactions. Even this amount of *lag* or *lead* in the reactions of *O*'s in relation to the instants of completion of each cyclic phase shift is adequate to *differentiate* Group 4 from any one of the other four groups. This differentiation is more significant, of course, for a comparison between Groups 1, 2, and 3. Possibly three factors operate in this time-error of the *O*'s; (*a*) actual differences in reaction times, (*b*) anticipation of the completion of a periodicity to which the reactor has become conditioned through serial performance, and (*c*) lagging or slightly delayed reactions to a region of change not sharply emphasized in a continuous variation. This latter point was clearly demonstrated in the practice periods, when the time-duration of one cyclic phase shift was increased to six or eight seconds. Here the instant of maximum loudness was evidently more difficult to discriminate, for the time-error range was 2.5 to 3.0 times the *percentage* error range for the four-second duration.

Interpretations as to the relative *directions of change* in the four

components and the *periods* of the variations can be readily interpreted from the graph. At the time *A*, tone 1 has attained maximum loudness; tone 2, minimum; tone 3, maximum; tone 4, maximum. At the time *B*, tone 1 has attained minimum loudness; tone 2, maximum; tone 3, minimum; tone 4, maximum. At the time *C*, tone 1 has attained *again* maximum loudness; tone 2, minimum; tone 3, maximum; tone 4, maximum.

During one cyclic phase shift for the *H*-component, tones 1, 2, and 3 have *one* period of loudness variation; but tone 4 has *two*. The variations in tones 1 and 3 are synchronized both in direction and rate; and tone 2 varies in the opposite direction during the same period of time. Alternate periods of maximum loudness in the tone 4 are simultaneous with instants of maximum loudness for tones 1 and 3.

The relation between *direction* of discrimination change and the phase conditions existing can be read from the graph. The instants *A* and *C* are phase condition III (coincident maxima), and *B* is phase condition I (coincident minima). The writer does not lay much stress on the accuracy of this calibration, however, since we determined the relation by judging visually the setting of both sets of inductor poles exactly opposite the center of the rotor poles yielding a condition of coincident maxima. We are devising a cathode-ray oscillograph for calibrating the phase condition, which will be more accurate. However, a fair approximation, perhaps within 10% error, is obtainable at present.

At phase condition I, then, tone 4 is at maximum loudness; tone 3, minimum; tone 2, maximum; tone 1, minimum.

From these records and this method of analysis we have been able to specify:

(*a*) what components of the compound varied independently; (*b*) the relative directions and periods of the variations; (*c*) the relation between rate and direction of loudness change, and the cyclic rate of phase-shift; (*d*) the relation between direction of loudness change and phase condition existing, and (*e*) the relative effect of *Einklänge* beats and the conditions specified for Smith's beats.

These five factors were not isolated and controlled in any previous investigation concerned with this problem.

Lindig (16, p. 268) concludes, after a review of the experiments preceding his own and a consideration of the positive results from his observations, that "Der einzige Fall, wo Phasenverschiebung

direkt hörbar wird, ist demnach der Einklang." By *"Einklang"* Lindig means "identical frequencies." When the phase is changed periodically between these, periodic variation in the r.m.s. energy value of this component varies between zero and a maximum of twice the r.m.s. value of a single component (if they are of equal intensity). Since this is the only artifact which any of the investigators of this problem have isolated, it is pertinent to ascertain if the present results could be explained if we assume that such conditions exist in the compound sounds we used.

This may be readily decided from a consideration of the details in Table 1. The harmonic overtone series for the two fundamentals we used (ratio 2:3) are as follows:

2	4	6	8	10	12	etc.
3	–	6	– 9	–	12	etc.

The only possible harmonics to enter into our problem are the ones having the ratio term values 6 and 12. This is determined by the *rates* of the loudness variations discriminated by the *O*'s, concerning which there were *no ambiguities* in our records. As the phase is retarded by 180° for the component 3, the phase of the harmonic 6 would be retarded by 360°, and that of 12 by 720°. Thus, for *one cyclic phase* shift for the 3 component, there would be *one* increase and diminution in the r.m.s. value of 6 and *two* such changes for the harmonic 12. On the mere question of *rate* this agrees with observation and is the *raison d'être* for Lindig's and others' interpretations. But there are numerous other considerations in which disagreement of observation with this manner of interpretation may be found.

On the side of hearing, this condition should appear as: The fundamental 3, frequency level 1440 cycles; the fundamental 2, frequency level 960 cycles; both fundamentals constant in loudness; the weak harmonic 6, frequency level 2880 cycles, varying in loudness once per cyclic phase shift; the still much weaker harmonic 12, frequency level 5760 cycles, varying in loudness twice per cyclic phase shift. When this is compared to the discriminations made by the *O*'s (*supra*), it is obvious that the two sets of conditions are significantly dissimilar.

Table 1 is largely self-explanatory. By comparing the loudness conditions which would be discriminated according to Lindig's hypothesis with those actually recorded in these experiments, we find too many disagreements for one to be considered equivalent to the

other. In the table the symbol *C* indicates constant; *max* is *maximum; min* is *minimum; inc* is *increasing; dim* is *diminishing.* However, in the absence of a technique for showing (*a*) which components vary simultaneously, (*b*) the direction of the changes in each, and (*c*) the relative durations of these changes, the mere agreement as regards *rate* between observation and the conditions supposed by Lindig is an inadequate basis for interpreting the results.

TABLE 1

A Comparison of Stimulus and Discrimination Conditions Due to Harmonic Interference with the Actual Discriminations Made by Sixteen Observers

Phase cond.	Partial ratio	Intensity condition if *Einklänge* 12 and 6	Loudness condition if *Einklänge* 12 and 6	Loudness condition actually discriminated
	12	max	max	inaudible
	6	max	max	inaudible
	4	C	C	max
I	3	C	C	min
	2	C	C	max
	1	C	C	min
	12	min	min	inaudible
	6	dim	dim	inaudible
	4	C	C	min
II	3	C	C	inc
	2	C	C	dim
	1	C	C	inc
	12	max	max	inaudible
	6	min	min	inaudible
	4	C	C	max
III	3	C	C	max
	2	C	C	min
	1	C	C	max
	12	min	min	inaudible
	6	inc	inc	inaudible
	4	C	C	max
IV	3	C	C	dim
	2	C	C	inc
	1	C	C	dim
	12	max	max	inaudible
	6	max	max	inaudible
	4	C	C	max
V	3	C	C	min
	2	C	C	min
	1	C	C	min

Summary

1. Conclusions drawn by former investigators regarding the effect of phase on tone quality are shown to be ambiguous, since the various techniques employed failed to take account of (*a*) which specific components of the clang varied and in what respect, (*b*) the relation between rate of variation in the discriminations and the rate of phase change, and (*c*) the relative *directions* of change in the components.

2. Preliminary observations and analysis revealed that the only audible components in the compound used are representable by the ratio terms 1 : 2 : 3 : 4. The terms *2* and *3* are for the major fundamentals, *1* for the weak differential tone, and *4* for the weak first harmonic of *2*.

3. Special apparatus was used permitting continuous change of phase between the fundamentals, with independent control of intensity and frequency. Automatic recorders indicated on a polygraph record the cyclic rate of phase change.

4. Preliminary observations under the condition of continuous phase retardation of the fundamental 3 indicated that each component of the clang varied in loudness (or pitch?), but at different times, and in some instances at different rates. The total pattern of variations was much too complex for an observer to analyze directly. Consequently, a method was devised for obtaining a complete objective analysis of the variations in each of the components of the clang by means of sixteen observers, who were divided into four groups of four members each. The individuals in each group were practiced in reacting to variations in a specific component of the clang, to the neglect of the others. This task was difficult at first, but, after many practice periods, satisfactory differentiation was obtained.

5. From various pairings of these groups whose discriminations were recorded simultaneously on a polygraph, a synthetic graph was devised, showing the (*a*) rates, (*b*) directions, and (*c*) components associated with the discriminations each set of *O*'s made. This synthesis shows: For each cyclic phase shift, the components 3, 2, and 1 vary once in loudness, and the component 4 twice. The direction of changes in 3 and 1 is synchronous, but those in 2 opposite. The component 4 attains maximum loudness simultaneously with a maximum of 3 and 1, and a minimum of 2; also, since 4 changes twice as rapidly as the other three components, it attains a maximum again simultaneously with a minimum of 3 and 1, and a maximum of 2.

6. The only likely artifact (the presence of Lindig's *Einklänge*) is considered, but the discriminations recorded do not in any way, except *rate,* agree with those that would obtain if Lindig's thesis held. Consequently, we conclude that *phase* gives a real basis for the discriminations listed; however, the direct stimulus change discriminated is a periodic change in the instantaneous pressure value of the compound wave. This is discriminated as a variation in the *relative loudness* (or pitch?) of the components in the compound.

7. The results are inconsistent with a strict resonance-analysis function as traditionally postulated, since such analysis would necessitate, for the present considerations, changes in the r.m.s. energy values of the components of the compound sound. Changes in the instantaneous pressure value alone would not be discriminated, according to a resonance theory of the mechanics of the cochlea. These results, therefore, would necessitate the assumption of loudness variations being dependent on the phase relations of excitatory processes in the cortex, if a resonance theory is maintained; or, substitution of a different theory of the relation between peripheral excitatory processes and distributing centers in the brain. It is apparent, however, that the minimal excitatory changes consequent upon small increments in intensity, frequency, and phase are not markedly differentiated neurologically.

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LES RÉPONSES DIFFÉRENTIELLES À LA PHASE CYCLIQUE—LES VARIATIONS DES SONS COMPOSÉS

(Résumé)

On montre que les conclusions des investigateurs antérieurs à l'égard de l'effet auditif de varier la phase entre les composants d'un son composé où il se trouve des sons partiels en relation harmonique sont ambigues, puisque les différentes techniques employées n'ont pas tenu compte (*a*) des composants spécifiques du son lesquels ont varié et de la manière de cette variation, (*b*) de la relation entre la vitesse de la variation dans les discriminations et la vitesse du changement de la phase, et (*c*) des directions relatives du changement dans les composants.

Des expériences avec l'"audio-phaser" montrent qu'avec un composant binaire 2:3, ayant l'harmonique très faible 4 et la fréquence différentielle au "ratio level" I, chaque composant varie en intensité (quelques-uns ont identifié le changement comme hauteur), pendant que la phase est changée entre les fondamentaux 2 et 3. Sur un graphique synthétique des discriminations faites par seize sujets on montre que: pour chaque changement cyclique de phase les composants 3, 2 et 1 varient une fois en intensité, et le composant 4 deux fois. La direction du changement en 3 et 1 est synchrone, mais celle de 2 est opposée. Le composant 4 atteint l'intensité maximum simultanément avec un maximum de 3 et 1, et un minimum de 2; aussi avec un minimum de 3 et 1, et un maximum de 2.

On considère la supposition de la présence de l'interférence entre les fréquences identiques dans le composant, mais on montre que celle-ci n'a pas de valeur comme base pour expliquer les discriminations vraiment faites. Par conséquent, la phase donne une vraie base pour ces discriminations.

Ces résultats ne s'accordent pas avec le traitement traditionnel de l'analyse de la résonance par l'organe auditif. La supposition que la sommation corticale dépend de la phase des décharges afférentes ferait que ces résultats s'accordent avec les théories de la résonance. Ces résultats sont cependant explicables par d'autres théories, surtout celle de Max F. Meyer.

BEASLEY

DIFFERENTIALREAKTIONEN AUF ZYKLISCHE VARIATIONEN DER PHASE BEI MISCHLAUTEN

(Referat)

Die von früheren Forschern gezogenen Schlussfolgerungen über die auditive Wirkung bei Variation der Phase zwischen Bestandteilen eines Mischlautes der harmonische verwandte Teiltöne (partials) in Anspruch nimmt werden als zweideutig erwiesen, da die von verschiedenen Forschern gebrauchte Verfahren nicht berücksichtigten: a) welche besondere Bestandteile des Klanges variierten, und auf welche Weise; b) das Verhältnis zwischen den Variationen bei den Unterscheidungen und der Schnelligkeit des Phasenwechsels (phase change), und c) die relativen Richtungen der Änderungen der Bestandteile.

Experimente mit dem Audio-phaseninstrument (audio-phaser) erwiesen, dass bei einem Mischlaut mit zwei Bestandteilen in dem Verhältnis 2:3 mit dem sehr schwachen Einklangston (harmonic) 4 und wo die differentielle Frequenz (differential frequency) auf dem Verhältnis-niveau (ratio level) 1 stand, jedes Bestandteil in Bezug auf Hörbarkeit (loudness) (manche

beschrieben auch die Änderung als eine Verschiebung der Tonhöhe) verschieden ist während die Phase zwischen den Grundtönen (fundamentals) 2 und 3 verschoben wird. Eine graphische Darstellung der von 16 Beobachtern (Vpp.) gemachten Unterscheidungen erwies folgendes: Bei jeder zyklischen Verschiebung der Phase (cyclic phase shift) änderte sich die Höbarkeit der Bestandteile 3, 2, 1, je ein Mal, und die Hörbarkeit des Bestandteils 4 zwei Mal. Die Richtung der Änderung ist bei drei und 1 synchronistisch, aber bei 2 entgegengesetzt. Der Bestandteil 4 erreicht seine maximale Hörbarkeit simultan mit einem Maximum von 3 und 1 und einem Minimum von 2, ebenfalls mit einem Minimum von 3 und 1 und einem Maximum von 2. Die Annahme, dass zwischen identischen Frequenzen in dem Mischlaute Interferenz bestehe, wird berücksichtigt, wird aber als Basis einer Erklärung der wirklich vollbrachten Unterscheidungen als unhaltbar bewiesen. Folglich liefert die Phase eine echte Grundlage für diese Unterscheidungen. Diese Befunde wiedersprechen der traditionellen Behandlung der Analyse der Resonanz durch das Organ des Gehörs. Nimmt man an, dass die kortikale Summierung von der Phase der afferenten Enladungen (discharges) abhängig ist, so werden diese Befunde mit Resonanztheorien in Einklang gebracht. Diese Befunde sind aber auch durch andere Theorien —namentlich die Theorie Max F. Meyer's—erklärbar.

BEASLEY

PAIN ADAPTATION: A CONTRIBUTION TO THE VON-FREY–GOLDSCHEIDER CONTROVERSY*

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E. F. Wells and L. B. Hoisington

Historical Introduction

The phenomenon of pain adaptation has received very little systematic study. Indeed, the fact itself is relatively new to psychology, although isolated instances of adaptation, discovered and reported incidental to other investigations, date as far back as 1894 (9, p. 193). Thus far the attack has come from two distinct directions: on the one hand, from physiologists, who are interested in pain adaptation only as tributary to their general theories of pain sensibility; and, on the other, from psychologists, concerned primarily with determining correlations between pain adaptation and external conditions.

The first group of investigators, represented by von Frey and Goldscheider, made their observations in the light of preconceived theory, and their results conform consistently to their respective theories. Von Frey finds that, under continued weak stimulation, cutaneous pain may become weaker and weaker until it finally disappears, without undergoing any qualitative change (10, p. 242). Goldscheider, on the contrary, repeatedly insists that cutaneous pain, in adaptation, goes over to a new quality—is replaced by a new quality—which is not painful. This new quality is *"einer unterschmerzlichen Empfindung"* or *"Druck"* (2, p. 289; 3, pp. 20, 71). Thus, the physiological attack has yielded two apparently irreconcilable sets of facts.

The phenomenon has received more direct treatment by psychologists. But the work done has been confined entirely to the determination of the conditions under which pain adaptation occurs, and the time intervals of adaptation with variation of these conditions (5, 7). The psychological approach has failed to yield a solution to the von-Frey–Goldscheider controversy.

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Problem

The conflicting results of von Frey and Goldscheider provide the point of departure for the present work. We sought to make a careful descriptive study of the course of pain adaptation with the view of determining precisely what becomes of pain as it adapts. In the results of such a study, we hoped to discover the reason for the apparently contradictory facts previously reported.

Specifically, the question raised at the beginning was, What are the phenomenological changes which take place in a pain experience as it changes from "pain" to "not pain," that is, as pain adapts? Later, in the light of our early results, we decided to broaden our problem so as to take account of the changes that occur subsequent to pain adaptation. Our task, then, as finally formulated, was to describe the course of pain experience from its maximum through its subsidence, without attempting to draw any absolute line between the process of pain adaptation proper and the processes subsequent to pain adaptation.

Experimental Work

Our experimental work fell into two distinct parts, which we shall designate as Series I and Series II, respectively. The general procedure and the instructions were identical in the two series, but the specific mode of stimulation and certain details of technique varied.

General Procedure. The cutaneous surface selected for investigation was a small area upon the volar side of the forearm, the exact location of which varied somewhat with different *O*'s according to the general conformation of the arm and distribution of superficial blood vessels. At the beginning of each observation period, the skin was softened with soap and water (in the earlier part of the work) or with pure glycerin (in the later part). The glycerin proved to be much the superior agent because of its slow rate of evaporation. One application kept the skin soft and pliable throughout the experimental hour. The *O*'s arm rested in a plaster cast, which prevented reasonably well any shifting of the skin during observations.

Observers. Our *O*'s were: Dr. L. B. Hoisington (*H*), Assistant Professor of Psychology; Dr. C. A. Dickinson (*D*); Miss F. B. Woolsey (*W*)[1] a graduate student in psychology; and Miss M. E. Farr (*F*), a student interested in psychological work. *H* had had

[1] *W* observed only in Series I.

long experience in psychological observation. *D* and *W* had both done some introspective work previously. *F* was an untrained observer.

Instructions: Our instructions, as finally formulated, were:

> "You will have set up a cutaneous experience. Take a passive receptive attitude toward it. Report on the experience with especial reference to qualitative development and subsidence during its course."

In the case of *D*, who had difficulty in giving qualitative reports in other than visual terms, the last phrase was changed to "with especial reference to the dimensional changes throughout its temporal course."

Stimuli. The ideal stimulus for work of this sort must fulfil three conditions: (*a*) It must exert a constant pressure for relatively long periods of time. (*b*) The pressure value must be subject to precise measurement and variation. (*c*) The stimulation must be confined as far as possible to the immediate vicinity of the point of physical contact.

Preliminary experimentation with the stimuli used by von Frey and Goldscheider convinced us that they did not meet these conditions. It was found that the von Frey hairs exert their maximum pressure just before they begin to bend, and that there is a slight variation of pressure thereafter with the degree of curvature.[2] This slight change of pressure value introduced two uncontrolled variables: variations in pressure value for any given observation, and variations in pressure values used for different observations (since it is practically impossible to maintain a constant degree of curvature in all cases). These uncontrollable factors completely invalidated this instrument for our purposes. Goldscheider's forceps proved even less satisfactory as a pain stimulus. Not only is it impossible to control the intensity of the pressure exerted, but the pinching of the skin causes varying degrees of tension in the surrounding areas, thereby introducing unknown factors and violating the third requirement mentioned above.

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[2]See (9, p. 186; 10, pp. 209 ff.; and 2, p. 289). The pressure values of the hairs were determined by von Frey's method (10, pp. 210-211). Our findings differ from von Frey's. According to von Frey, a resistance, which is close to the maximal pressure value of the hair, is reached before the hair is noticeably bent, but there is a slight progressive increase of pressure value as the degree of curvature is increased (10, pp. 210-212). The pressure values of the hairs which we used varied in the opposite direction.

A stimulus which fulfils all three conditions satisfactorily is the Head aesthesiometer (4). This stimulus was used throughout Series I. It was brought down upon the skin by means of Dimmick's (1) appliance for cutaneous stimulation. This mechanically controlled device made possible a relatively gradual constant rate of application and prevented any shifts in the position of the instrument during the period of stimulation, thereby eliminating variable errors due to manual manipulation. A constant stimulus intensity of 27.61 g., as measured on a pressure balance, was used throughout this series, that value being supraliminal for all our *O*'s.[3] The period of stimulation was 60″. This period was followed by a 10″ interval, at the end of which *E* gave the signal for *O* to begin his report.

The Head aesthesiometer almost invariably aroused subcutaneous as well as cutaneous end-organs. The presence of subcutaneous experience obviously complicated the course of adaptation.

In Series II, we modified our technique so as to rule out subcutaneous experience entirely. The stimulus was a needle algesimeter, constructed upon the principle of the Head aesthesiometer. This instrument gave cutaneous pain without exciting subcutaneous end-organs. The complete elimination of subcutaneous experience was proven by tests upon the etherized skin.

Two other variations in technique, the need of which became evident in the course of Series I, were introduced at this point:

1. Adjustment of stimulus value to individual pain limens. An apparently wide individual variation in pain sensitivity suggested the advisability of adjusting the stimulus intensity to the various limens of the *O*'s. By so doing we hoped to obtain more uniform results. The individual limens for the particular area under investigation were determined in the following manner.

An area, 6 x 8 cm., on the volar forearm, was shaven, marked off into 2–mm. squares, and worked over at random with a stimulus of moderate intensity to locate regions of high pain sensitivity.[4] About 30 such regions, or "spots," were determined for each *O* and marked temporarily with indelible ink. They were listed as first, second, or third degree of pain sensitivity on the basis of subjective judgment. Later in the same day (after an interval of at least 2

[3]This relatively high intensity for an adequate pain stimulus is due to the bluntness of the instrument.

[4]These regions served not only as a basis for the determination of a limen, but also as landmarks for future work.

hours), the work of mapping was repeated. From the two maps thus obtained, a third map was constructed, containing only those "spots" reported in both tests as of first or second degree of sensitivity.[5] The "spots" thus verified were marked upon the skin with a 10% solution of silver nitrate to correspond to the third map, which *E* retained for reference and checking purposes.[6]

We then determined the "statistical limen" of the area, using the "spots" previously localized as points of stimulation. A modification of the method of limits was employed. The "spots" were worked over with a series of stimuli, differing by 1–g. intervals. The first stimulus of the series was sufficiently weak to give no pain responses from the group of "spots." The stimulus value was then regularly increased until a value was reached which elicited pain responses from all the "spots."[7] The data thus obtained were grouped in a frequency distribution. The "frequency" (F) of a given stimulus value (x) is the percentage of "spots" which give a pain response to it. The relation may be expressed by the formula:

$$Fx = \frac{N—n}{N}$$

in which N represents the total number of "spots," and n represents the number of "spots" which did not give a pain response to the stimulus value x. The median of this frequency distribution is taken as the "statistical limen." This "limen" is, then, the stimulus value, which, under the given conditions, elicits pain from 50% of the "spots" under investigation and no pain from the other 50%.

The stimulus values used in Series II were 150% of the individual limens, thus determined. They were 22.77 g. for *D*, 15.38 g. for *F*, and 5.2 g. for *H*. These values were below the subcutaneous threshold, previously tested by the etherization method.

2. *Adjustment of period of stimulation to individual cases.*

[5]Verification was possible in a large percentage of cases, but the subjective estimates of relative sensitivity frequently varied.

[6]The silver nitrate was not altogether satisfactory as a fixing agent. Aside from the fact that repeated applications were necessary, much trouble was caused by the tendency of the acid to set up an irritation, which was followed by scabbing, thus temporarily preventing work upon the immediate area.

[7]Descending series were not used because it was found that, after a "spot" had once given a pain response, its sensitivity was so increased for an indeterminate period of time that it gave pain responses to previously subliminal stimuli.

The second variation of technique was the adjustment of the period of stimulation to the widely variable time required for adaptation. The uniform period of stimulation used in Series I was abandoned in favor of one which was automatically adjusted to the adaptation time in each case. *E* gave the signal for report at the end of a 30″ interval. The stimulus was not removed. If pain adaptation was not complete at this time, *O* continued observation until all pain quality had disappeared. Constant stimulation was maintained until *O* indicated his readiness to report.

Final Report. At the end of the work, every *O* wrote out a final generalized report, which supplemented the separate reports of specific cases. This final report included: (*a*) a description of the typical course (or courses) of experience as it passes from "pain" to "not-pain," or from "pain" to "no experience"; and (*b*) a statement regarding his interpretation of instructions, *Aufgabe,* attitude, and any other variable factors noted.

Results

Our results are based upon the descriptions of 106 separate pain experiences (52 in Series I and 54 in Series II). Out of this total, we obtained 83 cases of complete pain adaptation within the observation period, 16 cases in which pain adaptation was either incomplete or doubtful, and 7 cases that showed practically no adaptation.

Comparison of the Results of Series I and Series II. The results of Series I differ somewhat from those of Series II due to the different mode of stimulation. Both the typical pattern of the initial pain experience and the typical course of adaptation show characteristic differences in the two cases.

The pain pattern in Series I is described as a point or center of pain (clear pain or pressury pain), surrounded by, or set in, a mass of pressure, which, in turn, usually shades off into diffuse contact; less often, into a fringe of ticklish, prickly, or tingly experience. The form of the pattern as a whole may be either irregular and jagged or comparatively regular, circular, cone-like, or cylinder-like.

> *Observer H.* (1)[8] Prickly pain, surrounded by a diffuse pressury contact. (2) Sharp pricky pain, ... small, compact, deep, ... almost a cylinder of experience. (3) Pain which had a slight fringe of pressure, going off into contact.

[8]When quotations from more than one report are included in a single paragraph, they are indicated by serial numerals.

Observer W. A gradual formation of a cone of pressures, and then a sharp point of the cone of pain. Around the edge of the cone and extending out on the surface was tingling.

In Series II, the pain pattern is much less complicated. Pressure plays a less important part in it. When pressure is present, it tends to be beaten up with the pain quality, forming a pressury-pain fusion. Sometimes pain is there all alone—just a little spot or wire of pain. At other times, the pain is surrounded by a fringe of pricky experience. In the first case, the spatial form of the pattern is rather smooth, regular, circular; in the second, it is irregular, jagged.

Observer D. (1) A bulky spread of dull red quality.[9] (2) A hard core, with irregular extents radiating out in all directions . . . . very irregular, jagged.

Observer F. (1) Concentrated pain in one small spot. (2) Pain in a very tiny spot.

Observer H. (1) Pretty good pain, a little pricky. The prickiness seems to lie around the pain. (2) A little wire of practically clean pain. (3) Pretty good pain . . . . slight pressure resemblance.

There are also analogous differences in the course of adaptation. In Series I, the process of adaptation consists largely in the replacing of pain by pressure. Pain decreases, getting weaker, less intense, more diffuse; and, at the same time, dense dull pressure comes in all around it, from the sides and from the bottom, gradually supplanting the pain, until experience is just pressure. This pressure, in turn, loosens up, diffuses, softens, loses its localization, and finally drops out of experience.

Observer H. Presently the thing began to get more pressury and the pain quality dropped out. But one doesn't observe the pain dropping out—rather the pressure coming in. Very

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[9] *D* presents an interesting case of visual-touch synaesthesia. Cutaneous experience, for *D*, is constantly paralleled by visual experience. The latter predominates. Regarding the mode of linkage, *D* says, "I have to get under the visual stuff to describe the other [tactual experience], which appears less emphasized." Again, he explains, "This greyness is seen very definitely, but it also corresponds to this pressure. The greyness of the pressure is seen."

For each of the common qualitative categories used by our other *O*'s in describing tactual experience *D* has a visual equivalent. Prick is a "bright spark quality." Pressure is "dull slatiness" or "putty grey." Contact is "a flimsier grey;" either a dark surfacy grey, hard and close-textured (dull contact), or a porous, lighter grey, soft and loose-textured (bright contact). Pain is "dull red."

soon there was just a diffuse pressure, which kept getting looser, less dense, more indefinite. It became less distinguishable at the margin, faded out, lost depth.

Observer W. Sharp, concentrated, keen pain, which seemed to rise . . . . then fall abruptly. . . . Then, fluctuation of dull, rather diffuse pressure.

In his final report, *H* summarized this type of adaptation: "The dimensional changes in the passage from 'pain' to 'no pain' were typically, starting from a pain set within a surrounding pressure, of greater or less prominence, a spread of pressure inwards from all sides with a consequent shrinking of the pain. The pain also became less in vertical extent. At the same time, the experience loosened up, became less dense, and more on the surface."

In Series II, the changes in the course of pain adaptation are primarily changes in the pain experience as such. Pain gets weaker, less dense, less lively, and fades out. Sometimes it takes on a pressury resemblance as it fades. But, in these latter cases, pressure does not supplant pain; the pain quality itself changes in the direction of pressure. This type of adaptation will be treated more fully below.[10]

The only essential difference, then, between the results of the two series is that deep dull pressure tends to be prominent throughout the course of the experience in Series I, while it is absent in Series II. With the elimination of subcutaneous experience (Series II), the dull pressure component disappears. When this component is abstracted from the reports of Series I, the results are comparable to those of Series II. The two sets of observations may, therefore, be treated as a whole.

Classification of Pain Experiences. Marked differences of temporal course divide our data into two groups: first, those cases in which there is a single pain-maximum, followed by more or less continuous subsidence; and, second, those cases in which there are two or more pain-maxima, each followed by an interval of subsidence. The first group may be designated as *simple* pains, the second as *recurrent*.

Within the first group, two qualitative types may be distinguished: pure clear pains—pain experiences which are free from pressure; and pressury-pains—pain experiences which involve a pressure resemblance or accretion. The pure pains are bright, lively, wiry experiences; the pressury-pains, dull, volumic, less bright and lively.

[10]Page 362.

Observer F. (1) Quick sharp pain. (2) Dull pain.

Observer H. (1) Pure pain—a little wire of practically clean pain. (2) Not clear pain at all—a pressury-pain.

Observer W. (1) Clear keen pain. (2) Dull pressury-pain.

The second group, the recurrent pains, are highly variable. Two types of temporal course may be distinguished: the one characterized by two peaks or pain-maxima, rising out of a confusion of irregular minor oscillations; the other, by several maxima, recurring rhythmically, and usually decreasing in intensity as the series progresses.

In the first type of this group, the experience starts as a fine, wiry, clear pain (first maximum), which soon begins to adapt. Then, suddenly, while adaptation is going on, the experience again becomes painful (second maximum), a sharp, piercing, deep pain, often mixed with pressure, which gives it a dull, achy quality, that sometimes goes over to soreness. This second pain is more complex, more concentrated, more intense, than the first. It is uneven and oscillating, often decidedly unpleasant. Subsidence is slow, discontinuous, and irregular.[11]

Observer H. It began as a very fine wire of experience. . . . It was not long before pressure began to come in; the pain weakened and loosened. . . . Then it changed. Pain increased. . . . The experience became more concentrated, sharper, yet there was a good deal of pressure around it. . . . There was an increase in the total amount of experience, a big addition. It got very painful and unpleasant. Then, with time, there was a falling off of the pain.

Observer W. A stinging clear bit of pain, and then that deeper, more aching, duller pain, occurring suddenly but lasting. This subsided in intensity, fluctuating as it subsided. . . . The aching thing was more like a soreness than a pain.

Pains of the second type, the rhythmically recurring pains, are less variable and less complex, consisting typically of a series of pain experiences, each of which goes over toward pressure more or less uniformly in time, the pain itself decreasing in density, and taking on more of a pressury resemblance, as the series continues. This type of pain rarely adapted completely within the observation period. Frequently, it went over to a deep continuous soreness or ache.[12]

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[11]This type seems to correspond in some respects—its complexity, uneveness, and strong unpleasant tone—to Murray's (6) "hair-bulb pain."

[12]The serial recurrence of these pains suggests the possibility of correlation with some bodily rhythm, although we have not been able to establish such a correlation. Goldscheider has offered an explanation in terms of his "summation theory" of pain (2, p. 42).

Observer F. A pulsating pain. Pulsations were fairly regular and rapid. Toward the end of the experience, the pulsation became slower until there was no pain. The pulsations were alternations of pain and no pain.

Observer H. Rather pricky pain. The experience began to get pressury at the margins; it loosened up. After a little while, it got more painful again; pain shot down deeper; the whole experience was more concentrated. . . . Very quickly the pressure began to come in again. At the end there was only a hint of pain there. . . . One single pain went out at a uniform time, but there were successive waves of pain.

Observer W. A sharp, quick pain, occurring in regular rhythm. . . . It turned into a much more hard aching thing.

Out of the total 106 cases, 40 were simple non-pressury-pains, 39 were simple pressury-pains, 9 were recurrent pains of the duomaximal type, and 18 were recurrent pains of the multimaximal type. The simple pains adapted much more readily than the recurrent ones, giving complete pain adaptation in 91% of the cases, as against 41% for the latter.

The Course of Adaptation. The course of pain adaptation in Series I has been described and compared briefly with that in Series II. Only in the latter case, where subcutaneous experience is completely eliminated, may we properly speak of the process as *cutaneous* pain adaptation. We have now to consider in more detail the characistics of this process as exhibited in Series II.

Two distinctly different courses of pain adaptation are evident. In one, pain simply gets weaker, less dense, and fades out of experience. This may be called the non-pressury type of adaptation. In the other, pain takes on first a pressury resemblance as it fades, then merges into pressure—the pressury type of adaptation.

In the first, the non-pressury type, adaptation begins with a loosening of the pain-texture. Pain becomes less dense, less lively, less penetrating. Its intensity and volume gradually decrease. This process continues more or less constantly until presently pain simply fades out and is gone, leaving behind it momentarily a bright, surfacy, contact-like experience, which soon disappears. Pain is not supplanted, or crowded out, by a new quality; it simply fades off into a "bright contact" as it weakens. Sometimes, the *O*'s simply report that pain disappeared, without mentioning any non-painful end-quality.

Observer H. The experience became less definitely bounded,

> got a little looser in texture. . . . The direction of change was continuous, the pain experience getting less in extent, less intensive, still pain. It became weaker, less lively, less bright, less penetrating. That went on until pain disappeared.
>
> *Observer D.* That (pain) very quickly receded until there was no bulk (pain)—simply an extent. . . . It (pain) disappeared, seemed just to fade out.
>
> *Observer F.* Pain very gradually diminished until it was gone.

This non-painful end-quality is described as a "surfacy contact" a "dull brightness," a "bright lively contact." It is much brighter, livelier, less pressury-like, than ordinary contact (8, p. 146). It resembles the weak pain that precedes it, although it is not painful. It is more like pain than like pressure. Indeed, so close is this resemblance that the *O*'s found it difficult to say just when the transition from weak pain to "bright contact" occurred. The two successive phases are not sharply demarcated; the transition is not abrupt.

> *Observer H.* (1) The experience at the very end wasn't pain. It was much more like a light contact, but it was much brighter, livelier, less pressury, than ordinary contact. . . . It had the quickness of pain. (2) It (pain) got weaker and weaker and somehow less like pain in quality—seemed to be a very weak pain. It just diffused out to something that was a little brighter in quality, not painful, and then it was gone. (3) The whole experience just got less and less . . . . until presently there was only a bright lively bit of experience there that was somehow like pain, but it wasn't pain. It was more like pain than pressure. It is hard to say just when pain dropped out. The very end was simply losing it.
>
> *Observer D.* (1) Simply an extent which carried a certain brightness—a surfacy contact affair. (2) Change from dull redness (pain) to dull brightness (bright contact).
>
> *Observer F.* It (pain) seemed to spread in all directions and was gone. At the very end, there was a bright spot on the surface that wasn't painful.

In the second, the pressury type, adaptation begins much the same as in the other: a loosening of texture, spreading out in superficial extent, decreasing in depth and volume. Experience changes from something very concentrated, penetrating, and lively, to something that is less concentrated, less dense, and less lively. As these changes take place, the pain quality itself changes, taking on a pressury tone —it merges slowly into a pressure. This pressure, at first relatively

hard, dense, and massive, slowly becomes looser, more diffuse, less definitely localized, shading off ultimately into a diffuse, surfacy, dull contact. The qualitative sequence is roughly thus: pain, pressury-pain, pressure, dull contact.[13]

> *Observer H.* Experience lost its liveliness; the pain was a little less sharp. . . . The whole experience loosened up, diffused a little, became more pressury. Pain was soon gone. There was still one of those little indefinite pressures—like contact,
>
> *Observer D.* This extent changed from redness (pain) to a darker dullness (pressure), which seemed to lift, to become more surfacy (dull contact).
>
> *Observer F.* Pain, which quickly ceased and went over to pressure.

Thus, the two types of adaptation show characteristic differences in qualitative course. In one, experience passes directly from pain to "bright contact." In the other, the qualitative course is from pain to pressure to dull contact, the pressure constituting a conspicuous intermediate phase of the process.

The course of pain subsidence may be relatively smooth and continuous or extremely irregular and uneven. The cases in which the irregularities amount to actual recurrences of pain have been discussed above. In many cases, there are less marked irregularities, taking the form of little "starts" or "flares" of pain, which are quite different from true recurrences. *H* says of them, "These later experiences do not localize definitely like the original ones; they are phantom-like and floating." Minor irregularities in the course of pain subsidence are very frequent. In such cases experience changes by little steps, jerks, or jumps; it is not a smooth transition.

The absolute time of pain adaptation is highly variable. It varies with the individual, with the intensity of the stimulus, and with the spot stimulated. In general, simple pains adapt much more quickly than recurrent ones. With the stimulus values used in Series II, the rough average for simple pains was 30″; for recurrent pains, 100″. These figures are undoubtedly much larger than the actual time of the transition from pain to no pain, since they represent the interval from the moment of the application of the stimulus to the moment when the *O* indicated that pain was gone and that he was

[13]By "dull contact" we mean the ordinary contact that resembles weak pressure (8, p. 146).

ready to report. There is an indeterminant interval both at the beginning and at the end which should not be included in the time of pain adaptation proper. No attempt was made to eliminate this error, inasmuch as quantitative measurement was not the aim of our study.

Discussion of Results

Our results throw considerable light upon the von-Frey–Goldscheider controversy. To be sure, they do not enable us either to affirm or to deny the facts presented by these authors, for we have not repeated their work. But the facts which we have established, under somewhat different conditions, indicate that the quarrel is primarily the result of lack of precision in the use of descriptive terms.

Our results substantiate the contentions of both von Frey and Goldscheider to a limited extent. Like von Frey, we have found that pain subsidence may be primarily a matter of intensive change (our non-pressury type of adaptation). Like Goldscheider, we have found that pain may go over to *"Druck"* or to *"einer unterschmerzlichen Empfindung."* We disagree with both authors in our finding that the end-quality of pure, non-pressury, cutaneous pain (our "clear pain," supposedly aroused by the stimulation of isolated cutaneous pain spots relatively remote from pressure spots) is neither a weak pain (von Frey) nor a pressure (Goldscheider), but a non-pressury "bright contact."

The explanation of the variable course of adaptation is suggested by the fact that there is a fairly high correlation between the quality of the initial pain experience and the qualitative course of adaptation. Pressury-pains tend toward the pressury type of adaptation (88% correlation). Clear pains tend toward the non-pressury type (82%).[14] If we accept von Frey's theory that pressury-pains are aroused by simultaneous stimulation of pain spots and pressure spots, clear pains by stimulation of isolated pain spots, the same explanation may be applied to our two types of adaptation.[15]

Although interpretation and comparison of qualitative terms is always dangerous, we venture to suggest that Goldscheider's "subpainful sensation" and "pressures" at isolated pain spots correspond

[14]In computing this correlation, all cases of doubtful nature were classed as negative. If these cases were omitted, the correlation would be considerably higher.

[15]Time did not allow us to test out this hypothesis.

to our "bright contacts" and to von Frey's "weak pains." If such a correspondence actually exists, the situation immediately clears up. Von Frey's insistence that isolated pain spots give nothing but pain is, then, due to a confusion of "weak pain" with non-painful "bright contact." Goldscheider's contention that isolated pain spots give "pressure" is the result of a failure to differentiate between true pressure and the non-pressury contact-like experience which we have termed "bright contact." If von Frey will recognize that the final phase of pain subsidence is not really painful, and Goldscheider will distinguish between true pressure and "bright contact," the controversy will be settled.[16]

Summary

1. The course of cutaneous pain adaptation has been studied from a qualitative point of view: (*a*) when subcutaneous experience is present in the complex, and (*b*) when subcutaneous experience is eliminated.

2. Four types of pain experience are described: (*a*) simple non-pressury pains—a single peak of pure clear pain, followed by more or less continuous subsidence; (*b*) simple pressury pains—a single peak of pressury pain, followed by more or less continuous subsidence; (*c*) duomaximal recurrent pains—two peaks of pain (pain-maxima) rising successively out of a confusion of irregular minor oscillations, each followed by pain subsidence; and (*d*) multimaximal recurrent pains—several peaks of pain (more than two) recurring rhythmically, usually decreasing in intensity as the series progresses, and sometimes going over to deep ache or soreness.

3. A cutaneous pain experience may take either of two diverse qualitative courses in passing from pain to no experience: (*a*) It may simply get weaker, less dense, less lively, and less volumic, fading off into a bright contact just before the end (non-pressury type of adaptation): or (*b*) it may take on a pressury resemblance as it weakens, merging into a pressure, which, in turn, loosens and diffuses into a dull contact (pressury type of adaptation).

4. These two types of adaptation show a high correlation with the two types of simple pain experience; the non-pressury type with the non-pressury pains, and the pressury type with the pressury pains.

[16]Von Frey has already admitted the possibility of pressure adaptation under certain conditions (10, p. 243; 11, p. 328).

5. The presence of subcutaneous experience has a marked effect upon the pain pattern and the course of adaptation.

6. The course of adaptation varies from fairly smooth and regular to highly uneven, irregular.

7. The absolute time of adaptation varies widely. It is dependent upon the individual, the intensity of the stimulus, and the spot stimulated. Simple pains adapt much more quickly than recurrent ones.

8. The discovery of the quality of "bright contact" as the end-phase in the adaptation of simple clear pain provides an adequate basis for a resolution of the von-Frey–Goldscheider controversy.

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L'ADAPTATION À LA DOULEUR, UNE CONTRIBUTION À LA CONTROVERSE VON FREY-GOLDSCHEIDER

(Résumé)

C'est une étude descriptive de l'adaptation à la douleur. On décrit quatre types de douleur: (1) douleur simple et claire, (2) simple douleur due à la pression, (3) douleur duomaxima récurrente, et (4) douleur multimaxima récurrente. Quand on élimine l'expérience sous-cutanée, l'adaptation à la douleur peut suivre l'un de deux cours; ou douleur à douleur faible au "contact clair," ou douleur à douleur due à la pression au contact non clair. Les auteurs suggèrent que le premier type d'adaptation correspond aux résultats de von Frey; le second type à ceux de Goldscheider.

WELLS ET HOISINGTON

DIE ANPASSUNG AN DEN SCHMERZ: BEITRAG ZUR VON-FREY–GOLDSCHEIDERSCHEN POLEMIK

(Referat)

Wir haben hier eine deskriptive Untersuchung der Anpassung an den Schmerz. Es werden vier Schmerztypen geschildert: 1) einfacher, klarer Schmerz, 2) einfacher Druckschmerz, 3) duomaximaler wiederkehrender Schmerz, und 4) multimaximaler widerkehrender Schmerz. Wird subcutanes Erlebniss ausgechlossen, sokann die Anpassung an den Schmerz über zwei Wege stattfinden; entweder vom Schmerz über schwachen Schmerz zu "hellem" (bright) Kontakt oder vom Schmerz über drucktartigen Schmerz und Druck zu stumpfem Kontakt. Die Verfasser schlagen vor, dass der erste Anpassungstypus wohl mit von Frey's, der zweite mit Goldscheider's Ergebnissen übereinstimmt.

WELLS UND HOISINGTON

POSSIBLE BRAIN MECHANISMS AND THEIR PHYSICAL MODELS*

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In a previous paper (28) we have shown that, remaining entirely within the limits of well-established physical doctrines, we can imagine physical systems which behave under the influence of the surrounding in such a way that this behavior, if described in psychological terms, may be called learning, gestalt-response, and even thinking. We have emphasized very explicitly in the above paper that our aim was not to give any physical theory of mental processes, but rather to show that physical systems may exist, which possess certain characteristics of these processes. The kind of systems there considered is by no means the only one which could exhibit the necessary properties. The purpose of the present paper is to discuss systematically, as completely as possible, different kinds of physical systems, which may possess the property of learning, and to attempt to decide which of the possible schemes is most likely to be actually realized in the brain of the higher animals. Contrary to the preceding paper, this paper is, therefore, more an attempt at a physical theory of mental processes. As with all other theories, its value will be judged by the new experiments it suggests and by the degree in which the expectation of the theory will be verified.

I

In the preceding paper (28), we have considered learning as a particular type of hysteresis. The idea is not a new one (37), and this is quite natural, for if we define hysteresis in the most general way as being the dependence of the properties and behavior of a physical system not only on the instantaneous environmental conditions, but on all the previous history, then saying that learning, or

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more generally, all mental phenomena, are special cases of hysteresis is nothing more than a description of the most general characteristic of those phenomena in physical terms.

In the above-mentioned paper we have considered such kinds of hysteresis as are due to the existence of several equilibria in a thermodynamical system. As has been shown by the writer in several publications (29, 30, 31), this is not the only possible cause of hysteresis. Hysteretic phenomena will be produced in any physical system, whether it has one or several equilibria, whenever a quantity, characterizing the state of the system follows the variation of another quantity which specifies the variation of the environment, with a certain time-lag. It has been pointed out (29, 30, 31) that such time-lags occur in principle in all physical phenomena, so that hysteresis is a general property of all physical systems. However, in the majority of cases, the time-lags are very short, and therefore the hysteresis is vanishingly small. However, it must be emphasized that, from this point of view, the dependence of the reaction of a system on the previous history is rather a rule in physics than an exception.

There is an important difference, however, between time-lag hysteresis and hysteresis which is due to multiple equilibria in that the former kind vanishes with time while the latter does not (31). That is, when the environment is kept constant for a sufficiently long time, the system tends asymptotically to a definite state, which is a function of the constant environment only, which does not depend on the previous history of the system. The situation is illustrated in Figure 1, where the curve a_1 represents the variation of an external paramenter with time, and λ_2 represents the corresponding variation of the "state-parameter" λ (28). We see that, at the points t_a and t_b, the environments are the same, but the states of the systems are quite different. At t_c and t_d, on the contrary, the states of the system are the same in spite of different environments. But as a is kept constant from the time t_1, having the value of a_0, the state of the system, which is described by λ_0, approaches also a constant value, which is determined by the value a_0 only. The curve a_2 represents another possible variation of a, and λ_2 the corresponding variation of λ. We see that at the time t_1 the two λ's have different values in spite of equal a's, due to the effect of the previous history. But, as in both cases a is kept finally at the constant value a_0, both λ_1 and

λ_2 tend to λ_0, regardless to the differences between their previous histories.

It may be asked whether such a type of hysteresis may not be responsible for various mental phenomena. I have not attempted to work out the possible details of this case, but such a possibility could not be denied *a priori*. One would be particularly tempted to correlate the fact of disappearance of this hysteresis in time with the phenomenon of forgetting. One serious difficulty arises, however, in using this type of hysteresis for the explanation of mental phenomena. Although, in general, for a given environment, the state of the system will vary with varying history, nevertheless, different histories may lead to the same state, as seen in Figure 1, where at the point t_d both λ_1 and λ_2 intersect. And the worst of it is that in many cases the same state for the same environment may be reached in an *infinite number* of different ways. Many phenomena of response, involving the temporary sensitization of the organism by repeated stimulation (2, 20), closely resemble time-lag hysteresis with respect to their temporal properties; but, in the present article, in view of the above-mentioned difficulties, we shall omit further discussion of time-lag hysteresis and restrict our treatment to hysteresis which is due to multiple equilibria.

We have seen in the previous paper (28) that a system with only

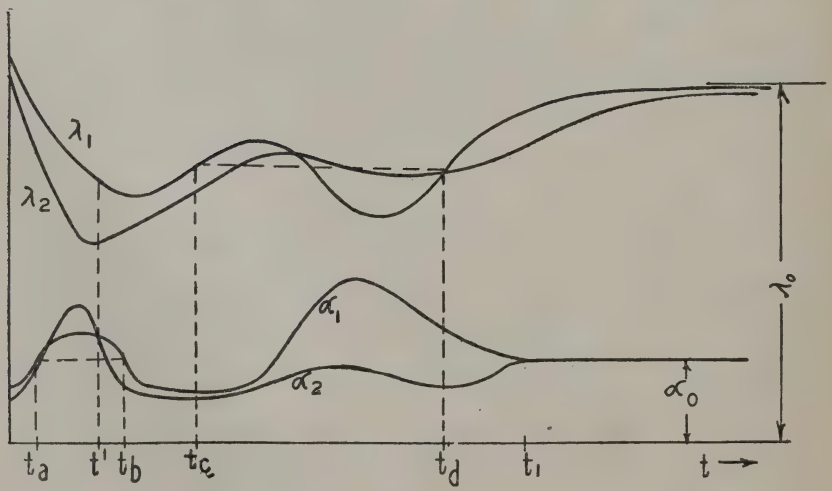

FIGURE 1

two equilibria gives us a complete scheme of a *simple conditioned reflex*. Moreover, by considering an aggregate consisting of a very large number of such two-equilibria systems, which are all alike qualitatively and differ only quantitatively, we arrived at a scheme of a physical system which shows the property of gestalt-discrimination; which reacts not on simple variation of the environment, but on a kind of "gestalt."

No special assumptions were made about the particular mechanism of the elementary systems, and the scheme was kept as general as possible. However, it must be pointed out that this scheme is not the only one possible for gestalt-discrimination. A different scheme has been elaborated by the author in a paper (30) which is based on the assumption of the existence of a separate elementary system for each possible pattern. Let us illustrate this by the following consideration. We have seen (28) that, whenever it is necessary to vary two parameters in order to bring a system with two equilibria from one state to another, the order of sequence of the two variations is essential. Consider now, as in the previous paper, a system consisting of *a*-systems and *e*-systems. Let the different *a*-parts be

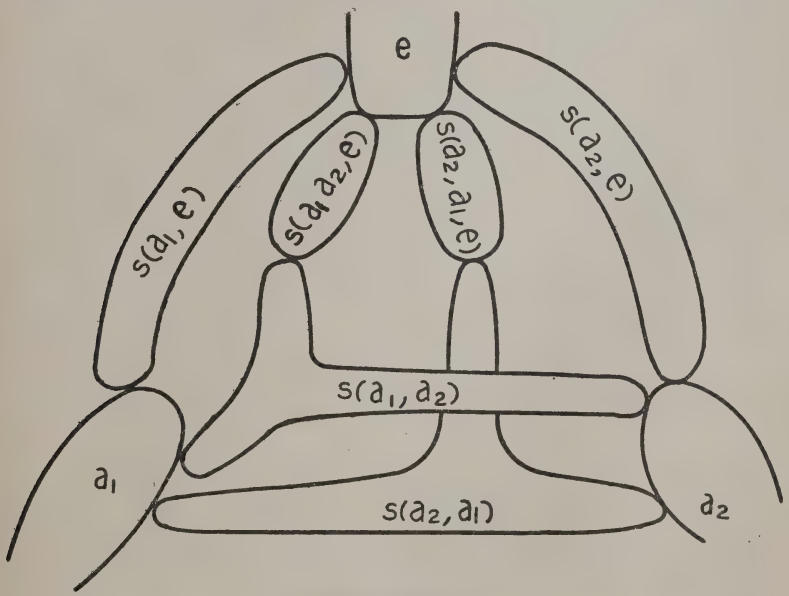

FIGURE 2

specifically different so that the chemical changes in each a-part are different. Let these systems be connected by means of several kinds of multi-equilibria systems, which we shall designate as $s(a_1, e)$; $s(a_2, e)$; $s(a_1, a_2)$; $s(a_2, a_1)$; $s(a_2, a_1, e)$; $s(a_1, a_2, e)$. (Figure 2.) These different s-systems are characterized in the following way:

$s(a_1, e)$ becomes transmittant when first a_1 then e are applied to it.

$s(a_2, e)$ becomes transmittant when first a_2 then e are applied to it.

$s(a_1, a_2)$ changes its state (which does not necessarily mean that it *becomes* transmittant, it may be transmittant in both states) when first a_1 and then a_2 are applied, but not otherwise.

$s(a_2, a_1)$ changes its state when first a_2 then a_1 are applied, but not otherwise.

$s(a_1, a_2, e)$ becomes transmittant when it is first influenced by the change of $s(a_1, a_2)$ and then by e.

$s(a_2, a_1, e)$ becomes transmittant when it is first influenced by the change of $s(a_2, a_1)$ and then by e.

All these systems are built according to the same scheme, namely, that discussed in (28) on page 217, only the nature of the parameters α and β are different in each of them. Thus, for $s(a_1, e)$ α and β are represented by a_1 and e. For $s(a_1, a_2)$ a_1 corresponds to α, a_2 to β. For $s(a_1, a_2, e)$ the change of state of $s(a_1, a_2)$, whatever its nature may be, acts as the parameter α, e corresponding to β, and so on. Consider now that we first influence a_1 then a_2 and finally a_3, which causes an unconditioned reaction in e. The result of this operation, if repeated several times, will be that $s(a_1, e)$, $s(a_2, e)$, and $s(a_1, a_2, e)$ will become transmittant, but not $s(a_2, a_1, e)$ because $s(a_2, a_1)$ did not change its state. If we now influence a_1 or a_2 alone, a reaction in e will in both cases follow via $s(a_1, e)$ and $s(a_2, e)$. We have a case of redintegration. But if this procedure of influencing a_1 and a_2 alone is repeated a sufficient number of times without reinforcement, $s(a_1, e)$ and $s(a_2, e)$ will again become non-conducive, so that, finally, influencing of a_1 and a_2 alone will not cause the reaction e. However, since the influencing of a_1 and a_2 alone does not cause any changes in $s(a_1, a_2)$, $s(a_1, a_2, e)$ will remain conductive. And if now the sequence $a_1\, a_2$ is applied, a reaction in e is set up, via $s(a_1, a_2)$ and $s(a_1, a_2, e)$. But the sequence $a_2 a_1$ will remain ineffective, because $s(a_2, a_1, e)$ has not been made conductive. The net result of influencing first $a_1 a_2$, reinforced by a_3, and then of non-reinforced influencing of a_1 and a_2 alone is the

response of the system to the sequence a_1a_2 only. By considering systems with more than two equilibria, it is possible to imagine a scheme of a system which will discriminate very complicated a-patterns. For mathematical details we must refer to (30).

If we now ask about the possibility of such a scheme occurring in the actual mechanism of the brain, several points have to be considered. First of all, the scheme requires physico-chemical specificity of the a-parts, or, in physiological language, of the afferent sensory neurones. Such a specificity has been postulated by physiologists for quite different reasons, and, in itself, does not constitute a drawback to the scheme (13).

Another point is much more important, namely, there must be at least as many elementary systems as there are patterns which the system as a whole could discriminate, and all these elementary systems must be qualitatively and structurally different. It would seem rather unnecessary to assume such a complicated structure, if the much simpler one discussed before leads to the same results. It is true that this simpler structure does not show redintegration, but we shall see in Section VI that this is obtained for a more general scheme of the same simple type.

Besides these general considerations, there seems to be strong experimental evidence against the complicated scheme and in favor of the previous one. Unless we complete the scheme still more by assuming that for each possible pattern there are several, so to say, spare elementary s-systems, it is clear that the destruction of a single s-system would render the discrimination of the corresponding pattern impossible. As each s-system must (on account of their tremendous number) be very small, this would mean that very minute lesions of the brain would cause a complete loss of ability to discriminate a certain pattern. Now, not only is there no experimental evidence of such a thing, but even more, Lashley's (23) experiments seem to indicate quite the opposite. We shall see that they are in complete agreement with the first scheme.

II

Recently, the idea of a kind of resonance as being responsible for the discrimination of various sensory impulses and patterns has been advocated, though no definite scheme, to our knowledge, has ever been suggested (19, 22, 34). The roots of this conception seem to

lie in the rhythmical character of the efferent (and sometimes afferent) nervous impulse. That a resonance between two periodical chemical reactions is mathematically possible, in this sense, that their mutual influence increases with decreasing difference of their proper frequencies, has recently been shown by the author (33). Such a phenomenon could therefore take place in the brain. In (28) we have considered the space-pattern due to physico-chemical fields around the *a*-parts. We may just as well consider the time-pattern at a fixed point due to the definite stimulus-pattern. If each individual stimulus, of which the pattern is composed, is periodic in time, the whole stimulus-pattern will also yield a periodic variation, say of the concentration of the *A*-substance (28) at each point. Now we could imagine some mechanism similar to that of a harmonic analyzer, which, finally, due to resonance, would cause each time-pattern to set in action a number of periodic reactions, representing the fundamental frequency and its harmonics, each with a proper amplitude. Thus a stimulus pattern $(a_i a_e a_m a_n)$ would set up reactions with the frequencies $\omega, 2\omega \ldots . n\omega$ so that the frequency $n\omega$ would possess the amplitude A_n. But in order to make this mechanism non-rigid, conditioned, a rather complicated scheme would be necessary. It would be at least necessary to assume that, in order that a reaction "*e*" takes place, all *n* periodic reactions should proceed simultaneously, and only with proper amplitudes. An elaboration of this scheme yields to the necessity of introducing *s*-systems of a type like that in (28). Thus we have a scheme which requires the same assumption as the first scheme, plus a lot of additional ones, without giving a sufficient justification for these additional assumptions.

III

Let us return to the scheme considered in (28). The fundamental principle involved may be stated as being the property of each *S*-system to become conditioned only to a definite value or range of values of the parameter whose field is created by the influence of the *a*-parts. Furthermore, we assume a linear arrangement of all the *S*-systems. This resulted in the circumstance that, as soon as the diffusion field deviated from the one at which the conditioning took place, even only in the region of a single *S*-system, the transmittancy of the chain of *S*-systems was interrupted, and no reaction followed. It

can be seen, however, that such a linear, one-dimensional arrangement of S-systems is unnecessary and that patterns can be discriminated also in an arrangement in two or three dimensions. We shall consider here the case of the two-dimensional arrangement, the arrangement being easily generalized for a three-dimensional case.

Let the plane between the different a- and b-parts be all filled with S-systems, Figure 3.

On Figure 6 of (28) we represented the distribution of the A substance by a curve, and each different a-pattern gave a different distribution. In the present case we must represent to ourselves the concentration as being given by the z-ordinates of a surface if the plane of the drawing is the x, y plane. To different a-patterns will correspond different surfaces. Just as the different curves of Figure 6 of (28) could intersect each other at a finite number of points, so the surfaces corresponding to different a-patterns will intersect along a finite number of lines. The projection of these lines on the x, y plane may have different shapes. Consider the case that we have conditioned the system to a definite pattern, say $a_i a_k$, and then apply the pattern $a_k a_i$. If the projection of the lines of intersection of the surfaces representing the concentration-fields for $a_i a_k$ and $a_k a_i$ have a shape like AB or $A'B'$ in Figure 3, it means that all the S-cells lying along AB or $A'B'$ will become transmittant also when the pattern $a_k a_i$ is influenced. But since these curves AB and $A'B'$ are closed and do not connect any of the a-parts with any of the e-parts, there will be no transmittant path between the a- and e-parts, and the pattern $a_k a_i$ will not cause any reaction e_r. However, it is conceivable that the projection of the line of intersection of the two surfaces will be like CD, Figure 3. Then, whenever $a_i a_k$ is conditioned, the application of the different pattern $a_k a_i$ also will cause a reaction e_r. But if we repeat $a_k a_i$ several times without reinforcement, all the S-cells along the line CD will again become non-transmittant. If we now apply $a_i a_k$ alone we find that all cells in the plane, except those lying along CD, are transmittant, so that the reaction e_r will take place. Thus, even for a non-linear arrangement of the S-cells, the system can discriminate complicated patterns. However, the case is now more complicated, inasmuch as the conditioning of a certain pattern may cause a conditioning of a few other patterns, so that, in order to make the system respond to only one pattern, it is necessary to repeat the other ones without reinforcement.

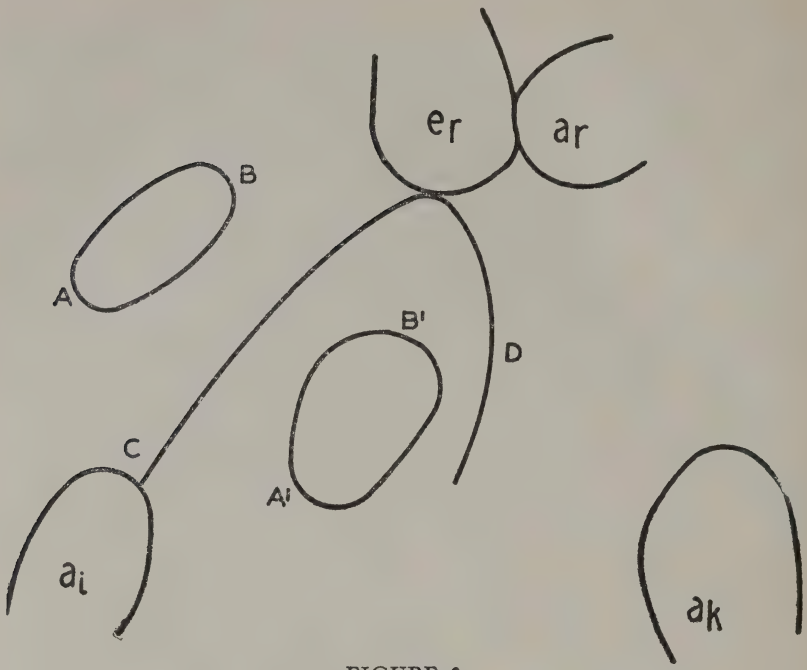

FIGURE 3

IV

In (28) we have emphasized that the exact mechanism of the *S*-systems is immaterial for the whole scheme. The essential general feature of an *S*-system is its composition of a great number of *s*-systems, all being qualitatively similar but differing with respect to their thresholds for a and β. We gave there, as a matter of illustration, the example of a colloid, each of whose particles possess two equilibria. We shall consider here a slightly different model, which is more easily visualized. It is understood, however, that this is also to be taken only as an illustration.

We have said in (28) that whenever an *a*-part is stimulated, in general, not one but several *A*-substances will be produced. Since their production is governed by one common reaction, their quantities will stand always in constant ratios. Suppose, as the next generalization, that we have two *A*-substances, A_1 and A_2.

Consider now an *s*-system, which consists of a solution L, sur-

rounded by a membrane M. Let the solution be a system with two equilibria, whose diagram is represented by Figure 3 of (28) and which does not need to possess double thresholds. A mathematical treatment of the equation of state of such a solution has been given elsewhere (32). Let, as in (28), the solution be transmittant in State II, and non-transmittant in State I, and let A_1 act as the α parameter, and E as the β parameter. The membrane M is supposed to become permeable to A_1 as well as transmittant under the action of A_2, and only when the latter is present in a narrow range of concentrations $c \pm \Delta c$. Such an assumption is physically quite plausible, for the transmittancy of the membrane to the particular reaction, whatever the nature of the latter may be, will depend, in general, on the permeability of the membrane to certain substances. But cases are well known (24) in which the permeability of a membrane to certain substances depends on the presence of one particular substance in such a way that the permeability has a maximum for a certain quantity of this particular substance. Let this substance be A_2. The permeability of the membrane is then represented as a function of the quantity c of A_2 by the curve in Figure 4 below. If the permeability p of the membrane must exceed a certain value p_0 in order that the membrane should become transmittant, we see that

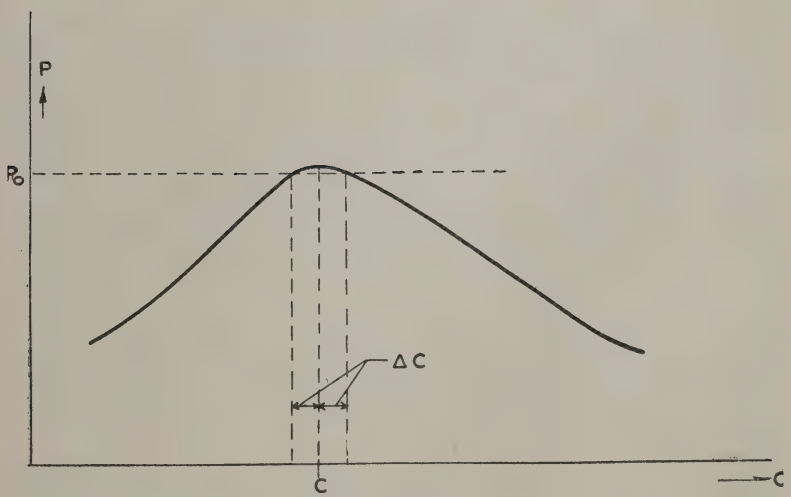

FIGURE 4

the transmittance will occur only for concentrations which are close to c.

Under the conditions considered, our s-system has this property. If L is in State I, the s-system is non-transmittant. If it is in State II, in which it is brought by the action of A_1 followed by E, it becomes transmittant, but only when A_2 has a definite concentration c_2^k, which, on account of the constant relation between A_1 and A_2, implies also a definite concentration c_1^k of A_1.

Consider now an S-system, composed of a great many s-systems, all of which have different values of c_2^k or else c_1^k, Figure 5, and are "connected in parallel." It is easily seen that such an S-system possesses[2] the required property of becoming conditionally transmittant only for definite values of A_1 and A_2. For, let the S-system be acted upon by a certain c_2^k or c_1^k, followed by E. A_1 will penetrate only into those s-systems which correspond to that c_2^k. Only in those s-systems will L become transmittant. When, afterwards, $A_1 + A_2$ act on the system again, even those s-systems whose L becomes transmittant will not transmit in general unless acted upon exactly by c_2^k, for M has become transmittant for this value only.

If, now, we put several such s-systems in series, we obtain a scheme discussed in the previous paper (28).

V

In view of the psycho-physiological applications, let us now consider how likely it is that such a scheme is actually realized in the brain.

First of all, as regards the production of different substances by brain cells during their function, there can be no doubt that such a production occurs both in afferent and in motor cells (37, 5, 11). Now, though we do not have any evidence of a difference in the distribution of these substances for different stimulus-patterns, there can hardly be a doubt that such a difference must exist. For, if the afferent neurons during the process of stimulation are sources of chemical substances, it is an unavoidable conclusion that whenever the sequence of setting these sources into action is modified, *the distribution of the produced substances,* whatever their nature, *must* be modified also.

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[2]It must be remembered that even the whole S-system must be so small that the concentration of the A-substances in all the region occupied by the S-system may be considered as constant.

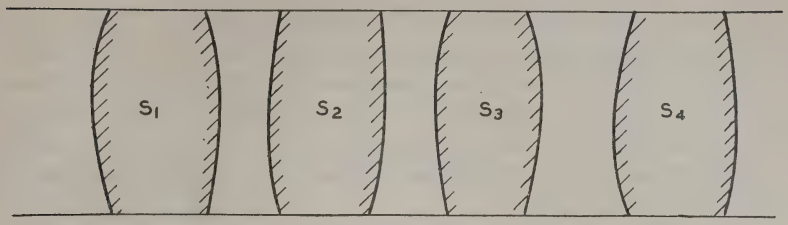

FIGURE 5

It must be remembered that attempts at detecting such diffusion-patterns are naturally found to be unsuccessful, if ordinary micro-chemical methods are used. What we are after is the instantaneous distribution of some substances shortly after the stimulus-pattern has been applied. If we should attempt to do this by killing the animal shortly after the stimulation and then fix and stain the brain, we would naturally find that, by the time the fixing agent has diffused through the tissue of the brain, the substances in question will also have diffused throughout the whole brain, so that the only thing that we may expect to find in this way is a general change of the micro-chemical staining reaction of the brain after stimulation, and this has actually been found (38). The suggestion may, perhaps, be made here that the brain could be cooled very rapidly by immersing the whole animal into liquid air, a procedure which will practically stop immediately all diffusion processes. The brain could then be sliced on a microtome cooled by CO_2 and the sections rapidly fixed and stained. No doubt such a severe treatment will seriously damage the structure of the cell tissue, but it may show the distribution of certain chemical substances in definite areas. The simplest animals which exhibit gestalt-discrimination should be chosen, and, since fishes show pitch-discrimination (8), they would perhaps be a proper object for such an experiment.

VI

Now we shall consider the question as to whether facts of brain cytology give any evidence of the existence of structures which may correspond to our *s*- and *S*-systems.

It follows from what has been said on this subject that an *s*-system may be characterized as being a system which is either non-transmittant, or is transmittant only for impulses of a quite definite

strength or when acted upon by a chemical of a quite definite concentration, which concentration-value we shall call critical. Furthermore, the transition from the non-transmittant to the transmittant state occurs upon action of the same critical stimulus or concentration followed by the action of another chemical. An *S*-system is essentially a group of *s*-systems connected in parallel and having different critical values.

If we now consider any pair of neurons, we see that they are connected by means of a very great number of end-brushes and dendrites, which results in the existence of a great number of parallel pathways between the two neurons, each pathway leading through a synapse (14). If we assume each synapse to possess the properties of an *s*-system, we may consider the whole set of synapses between two neurons as an *S*-system. The general cellular structure of the cortex would then immediately lead to the arrangement of *S*-systems discussed above.

Speaking physiologically, our assumption amounts to ascribing to each individual synapse the following property:

> Each synapse is capable of possessing one of the two states. In the first state it does not transmit any nervous impulse at all; in the second it transmits a nervous impulse only when acted upon by a quite definite concentration of a substance which is generated during the stimulation of any afferent neuron. The transition from the first state to the second one occurs when a synapse is acted upon exactly by the above-mentioned definite concentration of the above substance and when this action is followed by the action of another substance, which is generated during the activity of any efferent neurone. The definite concentration of the first substance varies from synapse to synapse.

From the foregoing it follows that such an hypothesis would account for the ability of the brain to respond conditionally to a very great variety of stimulus-patterns. We may now use the words synapses and *s*-systems, or neurons and *S*-systems indiscriminately. From what was said in (28), it follows that the brain is able to discriminate between the smaller differences in patterns, the more *S*-systems (neurons) it contains. Since this scheme represents the gross structure of the brain as being uniform, and since particular

patterns are not attached to definite structural units, it follows that removal of a portion of *S*-systems (neurons) will not destroy, in general, the ability or discriminating patterns, although when the removed part is so large that the total amount of *S*-systems (neurons) is appreciably reduced, the discrimination of very complex patterns, or, better, the ability to respond conditionally to very complex patterns, may be impaired.

On the other hand, when a number of *S*-systems (neurons) are removed, the total number of paths between the *a*- and *b*-parts becomes smaller, while the total number of *S*-systems (neurons) which can be brought into a transmitting state is also reduced. If, at each repetition during conditioning, a definite fraction of all *S*-systems (neurons) becomes conductive, and if a certain minimum number of conductive *S*-systems (neurons) is necessary to cause a complete reaction in an *e*-part, then one would expect that removal of part of the brain would increase the time necessary to learn to respond to a given stimulus pattern. Furthermore, the following important conclusion must be drawn from the scheme. The distribution of the *A*-substances depends not only on the distribution of the corresponding *a*-cells, but also on the shape and size of the boundary of the whole aggregate of *S*-cells, or, speaking physiologically, on the shape and size of the brain. Cutting away a part of the brain will have the result that a stimulus-pattern will now produce a distribution of *A*-substances, which is different from the one produced by the same stimulus-pattern before the operation. Hence the operation will cause a loss of previously established conditioned responses to various patterns, but, by a new relearning, these conditioned responses will be established again. All this agrees qualitatively with the facts observed by Lashley (23). Each synapse being specified by the critical amount of the first substance, as well as by the number and duration of repetition necessary to bring it from State I into State II, it is possible to work out mathematically the quantitative relations representing various phases of learning and thus subject the theory to the only real scientific test. This will be reserved for a next paper.

If we accept this scheme, we must conclude that each *a*-part is connected with each *e*-part not only through the intermediate of a series of *S*-systems (neurons) but also directly by means of a simple system having the property of Figure 3 of (28). We then can ac-

count for redintegration. For when a stimulus pattern is applied, it not only causes a corresponding change in the aggregate of *S*-systems, but each individual stimulus becomes conditionally connected to the afferent cell. These individual connections may then be removed by repeating the individual stimuli without reinforcement.

A few words may be said as to the possibility of an experimental test of our assumption about the synapses. The indirect test consists, of course, in the verification of the mathematical conclusions drawn from this picture of the brain mechanism. A direct experimental test will seem to be almost impossible. However, it must be remembered that cultivation of neuroblasts in vitro is now a well-known procedure (12), and recently cultivation of the adult cortical tissue has been successfully made (21). In such cultures, the excitability of single neuroblasts has been studied (27). In connection with the possibility of using micro-pipettes and microelectrodes for quantitative electrical measurement in the cell (9, 10), it may perhaps be not quite hopeless to study in vitro the property of a contact between two branchings of a neuron for the purpose of testing our hypothesis.

As regards any possible more detailed physical scheme which would account for the postulated property of a synapse, it seems to be premature to theorize too much on this subject, as we do not know enough about the mechanism of nervous conduction in general. It may suffice to have shown above what type of physical systems would show such properties.

VII

The simple two-equilibrium system discussed in (28) exhibits, amongst other properties of the conditioned reflex, that of experimental extinction, the system returning always to its original state when the conditioned stimulus is applied several times alone. This simple thermodynamical scheme, however, must now be slightly modified, for it does not account for an essential feature of experimental extinction. For it implies that a conditioned reflex, when once extinguished experimentally, should not reappear spontaneously, unless it is one of those reflexes discussed on page 225 of (28) which tend in general to become unconditioned spontaneously, whereas experiments on dogs (26) show that, as a rule, every well-established conditioned reflex reappears spontaneously after an experimental ex-

tinction. This peculiarity may be easily accounted for by a thermodynamical scheme, which involves only a slight modification of the one shown in Figure 3 of (28). The modified scheme is reproduced in Figure 6 below, the notations being the same as in (28). The only difference between Figure 6 of this paper and Figure 3 of (1) is that the curve $\alpha_1\beta_0$ still possesses a slight maximum in the former. We see again that if α is varied first from α_0 to α_1, so that we have the curve $\alpha_1\beta_0$, the system passes from λ_1 to λ_1'; and now when β is varied, so as to have $\alpha_1\beta_1$, the system passes into λ_2', and, upon return to the original values $\alpha_0\beta_0$, it will remain at λ_2. Similarly, as in Figure 3 of (28), the system will not pass from λ_1 to λ_2 if β is varied before α. But let us consider now the case in which the system is in State II (λ_2) and only α is varied so as to give $\alpha_1\beta_0$. The system will pass to λ_1'''' which corresponds to a non-transmittant state, but when α again takes the value α_0 the system will tend to reach λ_2 because λ_1'''' lies to the right of the maximum of the curve $\alpha_0\beta_0$. However, on account of the very small slope of this

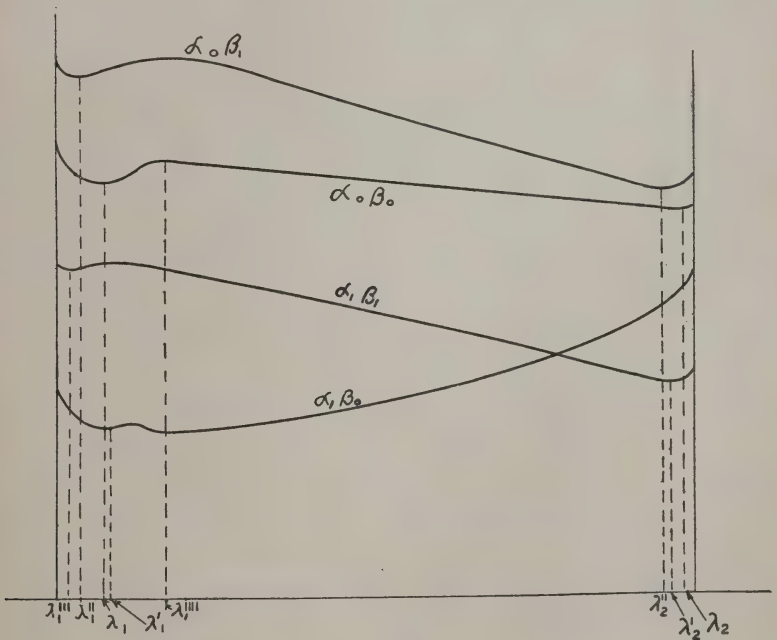

FIGURE 6

curve (28), the system will require a considerable time to reach λ_2 again. A mathematical treatment of this particular case of hysteresis will be given elsewhere.

The extinction of a reaction also can be interpreted as due to the development of an antagonistic reaction, if we accept the view (1, Chap. XVI) that to each reaction R there exists an antagonistic reaction $\overline{R}$; then the repetition of the conditioned stimulus without reinforcement means nothing else but the conditioning of this stimulus to the antagonistic reaction R (30). When finally the antagonistic reaction $\overline{R}$ becomes to prevail over $\overline{R}$, then R disappears.

In order to explain on this basis the spontaneous reappearance of the original reflex, some additional hypotheses are necessary. We may assume, for instance, that a reflex once established tends to strengthen spontaneously with time, the rate of this strengthening being proportional to the total time that the reflex is established. In this way the old reaction R will catch up the new antagonistic one and again become stronger. No such hypotheses are very satisfactory.

VIII

We shall now consider various properties which mechanisms of the above-described type may possess and which in many ways will correspond to different psychological phenomena. The arguments in the following paragraphs may still appear to be somewhat abstruse in the sense that the details of the physical picture of the mechanism considered will not be given, but only the general formal scheme will be considered. We shall finally cover this deficiency by describing an electromechanical machine which will possess all the properties discussed. This machine is to be considered, however, only as a help for visualizing the various relations, but in no way as representing a truthful model of the mechanism of the brain.

We have considered in (28) a phenomenon which may be called, in psychological terms, "thinking," and which consists in this, that the physical system considered reacts on a newly formed conditioned association of two afferent stimuli in such a way that, by associating them with such other stimuli as have been already "experienced" by the system, it substitutes for the new association-path a series of old ones, thus "explaining" a new association "in terms of old ones." We have considered as a basis for this process the presence of ther-

modynamical fluctuations which lead to temporary spontaneous formation of new paths between *a*- and *e*-parts. We saw also that physically there are two possibilities: either old paths are substituted for the new ones, or they are not. The first happens if the substitution of the old paths is connected with a decrease of the total free energy of the system. But whether such a decrease takes place or not depends on the details of the physical constitution of the system. In a system in which such a substitution is connected with an increase of the free energy, it will never take place. How would we describe the behavior of such a system in psychological terms? We would say that, whenever the system meets a new situation, instead of attempting to interpret this new situation in terms of old fundamental concepts, it tends to use the new concepts themselves as fundamentals. Instead of tending to reduce all possible concepts to a minimum, it tends to increase their number to a maximum.

It may not be out of place here to point out that this is what we actually observe in human beings, especially amongst "professional thinkers," scientists and philosophers. Psychologically speaking, is, indeed, the difference between mechanists and vitalists anything else but the difference in their propensities toward reducing new concepts to old ones? If the mechanists accuse the vitalists of an unscientific attitude in their introduction of entelechy, etc., is this "unmechanists? Does it not result from the definition of the word "scientific"? If the mechanist objects that the introduction of such concepts as entelechy does not explain anything, but only states that we have certain facts which we do not explain (or do not wish to explain), is it not merely an objection to a particular mental attitude to new facts? And if this attitude is really due to a certain constitution of the thinking apparatus, has such an objection any value? It is sometimes said that such an attitude will not help us to discover new facts, and, from this point of view, one may try to show a kind of superiority of the mental constitution of the mechanist. But the above statement requires great reserve. If it is true that the vitalistic attitude will hardly lead to discovery of new facts by prediction, nevertheless, a vitalist may just as well find those new facts by persisting in his search. Hans Driesch, the most eminent representative of extreme vitalism, is responsible for a great number of new discoveries in the field of ontogenesis. If we accept the physical interpretation of the mechanistic and vitalistic attitudes given above, we

see that the whole argument between the two groups becomes rather useless and unscientific. The correct procedure would be to consider both viewpoints as being defensible, and to investigate what biology would look like when either one or the other of the two doctrines should be developed completely and rigidly. We wish to emphasize that the above lines are not to be considered as expressing a defense of vitalism, such a defense being rather out of place in an article like this. The author himself is rather mechanistically inclined, but it is just this mechanistic interpretation of thinking which leads almost necessarily to the above attitude towards the problem of mechanism and vitalism. It must be remembered that physicists also exhibit a similar difference in mental attitude towards their problems, and probably a germ of such a difference will be found in any science. There remains, of course, the question: how can a vitalist accept the above interpretation?

The physical phenomena which we made responsible for this process of thinking were the thermodynamical fluctuations which caused spontaneous appearance and disappearance of different conditioned pathways. The simple scheme, however, can hardly be adequate for a real explanation. If the occurrence of the minimum-energy pathway is a mere matter of chance, why does it require an establishment of a new association for this minimum path to be reached? If a new association

$$a_i^* \rightarrow a_k^* \qquad [1]$$

is finally reached through the agencies of fluctuations by means of additional associations

$$a_i^* \rightarrow a_i \; ; \; a_k^* \rightarrow a_k \qquad [2]$$

to the old path

$$a_i \rightarrow a_k \qquad [3]$$

so as to give

$$a_i^* \rightarrow a_i \rightarrow a^k \rightarrow a_k^*, \qquad [3']$$

why, then, could [2] and [3], hence also [3'], not occur without the occurrence of [1]? If a thing like that occurs, we may perhaps say that, by a process of pure speculative thought, the system has predicted the possibility of [1].

A difficulty lies, however, in the circumstance that, in spite of possessing the minimum free energy, [2] will still be subject to

fluctuations and will not be entirely permanent. Perhaps an alternate picture will be more adequate in this case. The establishment of [1] through the action of environment, will, in general, be connected with a change in the total energy-content of the system. Now the existence of the alternative conditioned pathways is a function of the distribution of the different *s*-systems between their two states; their distribution is, in general, according to statistical mechanics, a function of the total energy-content. Hence the establishing of [1], or, in other words, the observation or discovery of a new relation, will cause a rearrangement of different conditioned pathways, and this rearrangement requires a finite time, which is in principle calculable from the constitution of the system. When this final new state is reached, we again will find that either the substitution of [2] for [1] has taken place or not. The rest of the interpretation is the same. While the system is rearranging itself, several other connections will be temporarily established, which may be said to be "no good" because they are not the final ones. The "trial" element in thinking, which was mentioned in (28), is thus preserved.

IX

In a recent paper, Hull (15) has discussed how the process of trial-and-error learning can be considered as a composite process which reduces finally to a sequence of conditioned reflexes. By applying Hull's reasoning to the systems which we have here considered, we see that they will exhibit, amongst other properties, that of trial-and-error learning also. There are, however, a few points in Hull's treatment which we shall discuss somewhat at length, since they raise certain difficulties and admit of several interpretations.

Hull assumes that a successful reaction is characterized by the circumstance that it is reinforced and thus becomes conditioned to an originally neutral stimulus, whereas an unsuccessful reaction gradually vanishes because it is not reinforced. But the reinforcement of a reaction means that, after the conditioned stimulus, an unconditioned one is applied. In other words, Hull's assumption implies that the reaction, if successful, will cause an "inner stimulus" (proprioceptive stimulus), which again will evoke the reaction unconditionally, according to the scheme of Figure 7, so that we have a kind of "feed-back circuit." Such a case is perfectly conceivable and may be particularly true for hunger reactions, in conformance with the

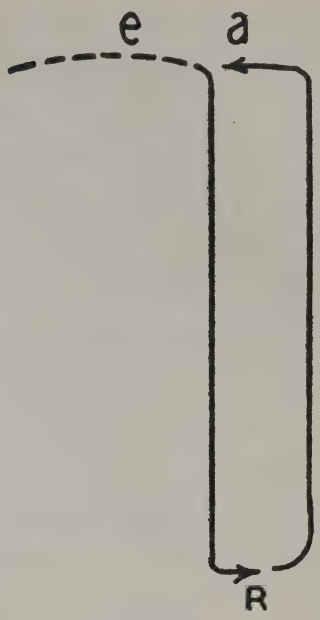

FIGURE 7

French proverb *"l'appetit vient en mangeant."* But it is somewhat unlikely that this scheme would hold quite generally, and in many other cases we must look for a different interpretation of a successful and unsuccessful reaction. Let us consider the following one.

The original neutral stimulus of an unsatisfied desire sets up in the organism a general activity causing various reactions which originally go on entirely at random. (We shall see later on how such an arousal of activity may be represented by means of a mechanical model.) If a reaction is unsuccessful, the stimulus persists and the activity of the organism continues so that sooner or later a reaction is made which is the opposite of the first one and brings the organism into the original situation (an animal in a maze, after attempts to pass through a cul-de-sac, will finally come back to its entrance), so that the net result of this sequence of reactions is to maintain the original situation, though this result is at first reached by quite a complicated series of reactions. By the principle of "short-circuiting" (16) or elimination of unnecessary actions, the stimulus will finally stop evoking that whole complicated series. If, however, a

successful reaction is made, the stimulus simply disappears; no further reactions follow and the reaction in question becomes alone conditioned to the stimulus because it has followed the stimulus immediately. Hull also assumes that the successful reaction terminates the stimulus; however, his more essential assumption is that the successful reaction is in some way reinforced, whereas the unsuccessful one is not. It seems that, together with the principle of "short-circuiting," the principle of elimination of the stimulus by a successful reaction is sufficient for the interpretation of the trial-and-error learning.

There remains, however, the problem of interpreting the "short-circuiting effect." This, however, has been done in another paper by Hull, and we believe that his interpretation by the "persisting core of sameness in the stimulus-complexes throughout the successive phases of the reaction sequence" (16) gives an entirely satisfactory explanation of the effect.

Another difference between Hull's conception and ours is that, whereas we assume that the stimulus of unsatisfied desire sets up reactions which go on at random, Hull assumes that the sequence of these reactions is usually predetermined by the previous history of the organism which results in a definite relative strength of the different "bonds" (16). This difference is, however, more apparent than actual. For if we attempt to treat the problem mathematically, we have to assume that these bonds are governed also by chance, since their formation in the previous history of the animal is a matter of chance only. An attempt to bring both conceptions into mathematical form and thus compare their points of similarity and difference will be made in another paper.

X

Hull has also given a remarkable interpretation of "foresight" and "purpose" which involves the principle that, when we have two chains of events, one external, the other produced by these external events proceeding in the brain, the latter chain may proceed at a much higher speed than the former so that the initial event in the external chain may cause a reaction to the final event in that chain before this final event actually happens. In the following, we shall consider somewhat more complicated cases of foresight and eventually such a case which may justly be called "invention," the latter being only a special type of foresight.

Consider an organism, more generally a physico-chemical system of a type discussed in this and in the previous paper, whose structure is given by the scheme of Figure 8. The scheme is a familiar one to the psychologists and has been used with different notations and some unessential differences in the literature (18, 7, 3). Any external event, for instance A, causes an excitation impulse to flow through a series of intermediate cells to the corresponding afferent neuron (a_A), which is unconditionally connected with a series of efferent neurons of which, for sake of simplicity, we show in the diagram only one e_A. The excitation of e_A causes the muscular reaction R_A, which, in its turn, excites the proprioceptive neuron a_A'.

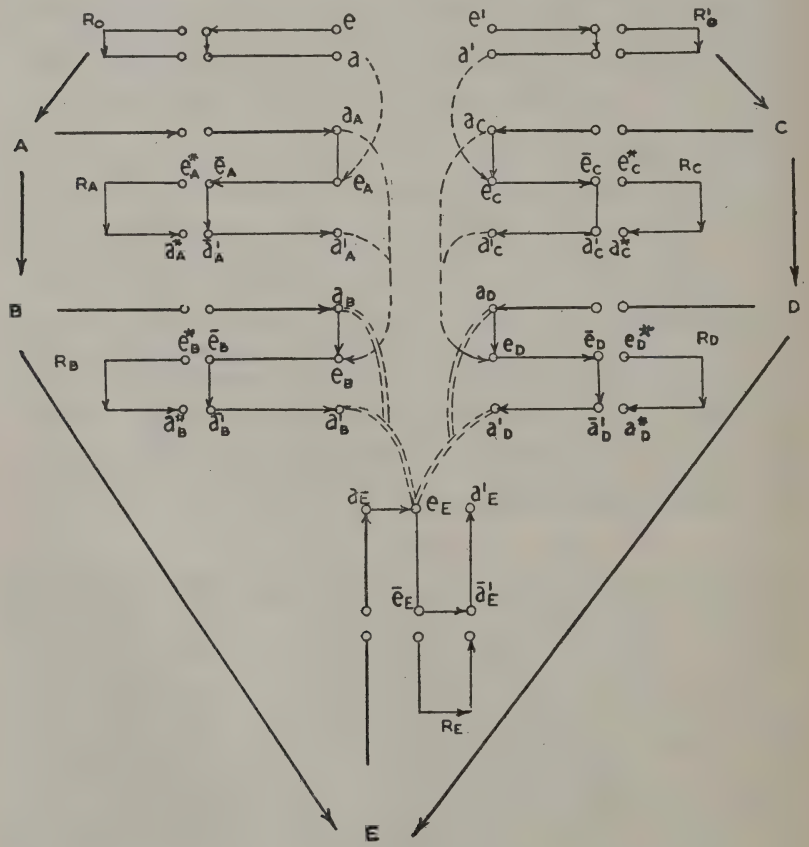

FIGURE 8

If the external events A, B, E happen in succession, the excitation of a_A and a_A' will precede that of e_B; the excitation of a_B and a_B' will precede that of e_E; so that, according to what we have seen previously, conditioned paths will be formed between a_A and a_A' and e_B, etc, as shown by dotted lines (the meaning of the double dotted lines will be explained later). In this way, after several repetitions of the series A, B, E, A alone will cause the sequence of reactions R_A, R_B, and R_E.

However, the presence of the intermediate S-systems ($\bar{e}_A$, $\bar{a}_A'$, etc.) makes it possible for e_A, e_B, etc., to be excited without the corresponding reactions R_A; R_B, etc., occurring, due to the short-circuiting between $\bar{e}_A$-$\bar{a}_A$, $\bar{e}_B$-$\bar{a}_B'$, etc. Only when e_A, e_B, etc., are excited very intensely will R_A, R_B, etc., actually occur. Speaking physiologically, we assume here that the excitation of a cortical motor neuron may not necessarily cause a complete reaction in the corresponding muscle, but may, so to say, reach only an intermediate neuron. Only if the excitation of the cortical motor neuron is supraliminal will the corresponding muscular reaction actually occur.[3]

Without entering here the question of the physiological nature of "desire" or "volition," let us assume that our system or organism possesses, besides the previously described structure, a mechanism of the following kind also. To each afferent neuron there corresponds a mechanism whose function consists in setting the different e-parts (motor neurons) into activity in random sequence. The action of this mechanism d_i is inhibited by the excitation of a'_i so that d_i is set in operation whenever a'_i has not been excited for a sufficiently long time, and it stops its activity as soon as a'_i is excited. Furthermore, the excitation of a'_i causes the excitation of a special a-part which we shall denote by $a\ (d_i)$.[4]

We shall not make here any hypothesis as to the nature of d_i. In general we may assume the existence of special neurons, but this is not absolutely necessary. We may, for instance, assume that the

[3]Since the excitation of the intermediate "neuron" which does not result in the "visible" reaction, R remains unnoticeable to the senses of an observer without the use of some special instruments, it would perhaps be thought of as the physiological reaction corresponding to thinking. Intense thinking would, according to that scheme, cause corresponding "visible" reactions. (Compare talking and gesticulating during intense thinking.)

[4]In case of hunger, as discussed by Hull (15), the cramps of the stomach would represent d_i, a'_i being the stimulus received when food is taken in; whereas $a\ (d_i)$ are the enteroceptive impulses, caused by the cramps.

above activity, which consists in the excitation of the various *e*-cells, sets up, as soon as $\bar{a}'_E$ is excited alone, without the excitation of $\bar{e}_E$. The activity stops whenever both $\bar{a}_E'$ and $\bar{e}_E'$ are excited, which always occurs upon excitation of e_E. In the following section we shall see how such a general scheme may be realized on an electromechanical model.

We assume, furthermore, that whenever d_i is stopped by excitation of a_i', the excitation of the corresponding e_k which caused the excitation of a_i' does not stop immediately but lasts for a finite time.

Now suppose that several times during the previous history of the system or organism, when the organism made a reaction *e*, this reaction caused the appearance of the external event *A*. As a result, a conditioned path will be formed between *a* and e_A, as shown by the dotted line, so that excitation of *e* will cause an excitation of e_A and a_A'. In a similar way, let a' become connected with e_C. Suppose, further, that the external event *A* is necessarily followed by the external event *B*, and the external event *C* by the external event *D*. Then, according to what was said above, we shall have also conditioned connections between a_A, a_A', and e_B; a_C, a_C', and e_D. Let the events *B* and *D* not cause any further events when occurring alone, but, when occurring simultaneously, cause the event *E*, and assume that in the previous history of the organisms $(B + D)$ occurred several times without the previous occurrence of *A* and *C*. Then we shall have a conditioned pathway for the pattern $(B + D)$ which we indicate by a double dotted line between a_B, a_B', a_D, a_D', and e_E. If in its previous history the organism has had the chance of performing the reaction *e* and *e'* simultaneously, then, since a simultaneous occurrence of *A* and *C* would have caused the occurrence of *E*, there would exist also other direct conditioned pathways between *a*, a', and e_E. But we shall consider the case in which such a thing *did not occur during the previous history of the organism.* Using somewhat more anthropopsychic expressions, we may describe this by saying that the organism "knows from direct experience" that *A* is always followed by *B*, *C* by *D*, and $B + D$ by *E*. But it does not know from direct experience that $A + C$ is followed by *E*, since this particular kind of experience *has never occurred.*

Now let us consider the case that for some reason d_E becomes excited. According to our assumption, this will set into excitation various *e*-neurons. Suppose, for instance, it excites *e* alone. The

excitation of e will cause the excitation of a, e_A, a_A', e_B, a_B'. But the excitation will stop here because e_E will react only to $(a_B' + a_D')$. But, in the sequence of these different "trials," let e and e' become excited simultaneously. The excitation of e will excite a_B', that of e', a_D'; $(a_B' + a_D')$ will excite e_E, and hence a_E'. If the excitation of the mechanism d_E is not very strong, the excitation of a_E' will terminate the "trials," and, according to our assumption, $(e + e')$ will last for a while before disappearing. By the principle of short-circuiting none of the former trials will have been conditioned to $a(d_E)$ (which is excited throughout the excitation of d_E), but $(e + e')$ will become conditioned. Hence, whenever d_E is again excited, the excitation will spread from $a(d_E)$ so as to cause $(e + e')$. If the excitation of d_E is very strong, the excitation of e and e' will also be so strong, that R_0 and R'_0 will be executed and thus cause $A + B$ and hence E to happen.

Describing the behavior of such a system in psychological terms, we may say that the system acted as follows:

It "knew" from experience that R_0 causes the event A, R'_0 the event C. It knew further that A causes B, and that C causes D. It knew also that $B + D$ causes E. Then, for some reason it began to "desire" to experience E. In "desiring" E, it performed an act of logical inference: Since A causes B, and C causes D, and since $B + D$ causes E, hence $A + C$ causes E. And since R_0 causes A and R'_0 causes C, hence $R_0 + R'_0$ will cause E. We must keep in mind that in the previous history of the system $A + C$, or $R_0 + R'_0$ has never occurred, so that "inference" will be the proper name to describe the above process.

This terminology is justified by the circumstance that the above process of trials occurred on a different level from the ordinary muscular reactions. The actual sequence of reactions R_0, R_A, etc., is produced only after the proper way of doing them is found on that other level.

These considerations of the above scheme, though very hypothetical, possess several interesting points. It hardly can be denied that an unprejudiced observer would call the behavior of the system "purposeful." All this raises also the question as to the nature of logic. In the present case the cause of the whole inference is entirely due to the particular structure of the system. Due to this structure, the inference will be made with absolute necessity. How-

ever, other structures would be possible in which such an inference would not take place. The question has been debated as to whether or not our Aristotelian logic is the only one possible (4; 35, Chap. VII). Ruark (38) assumes the possibility of other logics, and looks for their possibility in the structure of the external world. He attempts to give an example of such a fictitious world, in which a thing could exist and non-exist at the same "time." We believe that this can happen also in our own external world with a system which possesses the necessary structure. The perception of existence and non-existence of an object causes correspondingly the excitation of different (antagonistic) neurons. If, due to the structural nature of our brain, these two antagonistic neurons, or groups of neurons, cannot be excited simultaneously, we shall never be able to conceive an object as existing and non-existing at the same time. This will be logically impossible. But it may happen, in pathological cases, and it actually happens in dreams, when the brain mechanism is modified in some way, as yet unknown, that the two groups become excited simultaneously. It must be remembered that the logic of our dreams sounds absurd, when judged from the waking state. But while dreaming we find this logic, in which a thing may be and not be, quite perfect. The logic of the dream and of the insane is nothing else but such non-Aristotelian logics, and, as such, they are not only possible, but they exist. One may object that they do not exist in the normal state, and it may be replied that there is not always a sharp limit between normal and abnormal (36), or sane and insane. It may be argued that the application of non-Aristotelian logic to practical life will result in rather disastrous consequences, incompatible with the preservation of the individual and of the species. But we wonder whether the results would be less disastrous if a draftsman should make a drawing of a machine using the rules of non-euclidian geometry, and if the machine should be built according to such drawings; and yet the non-euclidian geometry *is possible.* The question at issue is therefore not the possibility of non-Aristotelian logics, but of the *practical* value of these different logics from the point of view of preservation of the individual.

It may be mentioned here that we cannot in a normal state think by rules of a non-Aristotelian logic, but we can think of non-euclidian geometry. This is true, but it must be remembered that we cannot completely visualize non-euclidian geometry.

Returning to the discussion at the beginning of this paragraph, we shall point to the important circumstance that a system which is made according to the scheme of Figure 8 could be used as an indicator of the past events, or, in other words, as a kind of recording apparatus. For, if the sequence of the events *A, B, E,* etc., has previously occurred several times, the occurrence of *A* alone will cause the reactions R_A, R_B, R_E to occur in the same sequence. If *A* was the starting event of another chain, say *A, D, E, F,* then the occurrence of *A* will cause another chain of reactions R_A, R_D, R_E, R_F, etc. As we said, every event will cause in general not one, but a great number of different reactions, and it may happen that the same reaction will be caused by different events. But if between all the reactions caused by an event there is a group which is caused by that event alone we may consider those reactions as a "symbolic expression" of this event. In considering now only such "specific" reactions, we see that from their sequence we may infer the sequence of the previous events. In a sense, we may consider the muscular reactions involved in speech as belonging to such a type of reactions (25). In this sense we may say that the system considered "tells" us through the reactions R_A, R_B, etc., of the past sequence of events *A, B,* etc.

In a very general sense we may say that every recording apparatus "tells us" of the past sequences of events. Inasmuch as every such apparatus may go wrong, we may say that it may once in a while tell a lie, although every one will agree that in ordinary recording apparatuses such "lying" is, as a matter of fact, purely accidental. But, in connection with the above-discussed "purposefulness" of the behavior of our system, let us consider the following case.

Let (Figure 9), in the previous history of the system, the event *C* have occurred, which caused the reaction R_C. This reaction, by influencing the environment, causes the event *D*.[5] Suppose that now an event *A* has been followed by the event *B,* causing the reaction R_B, which inhibits the event *D*. Now let d_D become excited simultaneously with the presentation of the event *A*. Without the excitation of d_D, the system would produce the reactions R_A, R_B. On the other hand, according to what was previously said, the excitation of d_E will result in a series of "trials" on the "covert level" with the

[5]It must be remembered that words as reactions may influence the environment of the person who pronounces them.

final tendency to perform R_C. If the second process predominates, the system will show the sequence R_A, R_C, instead of R_A, R_B. In our terminology, we may say again that the system "tells a lie," but this time the lie is not accidental. If, as we did above, we say that the excitation of d_D is a desire for D, we must describe the behavior of the system by saying that the desire for D caused it to tell a lie. In other words, the lie may be described as "purposeful."

Of course, as long as the mechanism d_E is hidden from the outside observer, there is no empirical way to discriminate between a purposeful lie, and an "accidental" one, like that of any ordinary instrument. But in their ultimate physical mechanism the two cases are quite different. And do we have any very sure criterion to decide, in the case of a person, whether he lies purposely, or simply due to a fault of his memory? The above discussion is, however, of importance in connection with a remark of Hans Driesch quoted at the end of this paper.

XI

We shall now consider a general scheme of a machine, which will possess all the above discussed properties. It is quite obvious that the more perfect such a machine should be, the more complicated must be its structure. We shall limit ourselves therefore only to a most general scheme, without touching at all the question of the actual design. What we are interested in is the principles on which such a machine should work.

We have seen that all the phenomena considered above reduce ultimately to two fundamental processes: First, to the conditioning to definite stimulus pattern, and, secondly, to the appearance of a "spontaneity of action" which causes the trials, whether they be "physical" or "mental." The first principle resolves itself into a com-

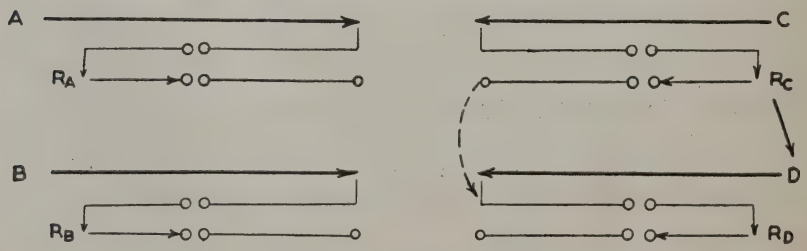

FIGURE 9

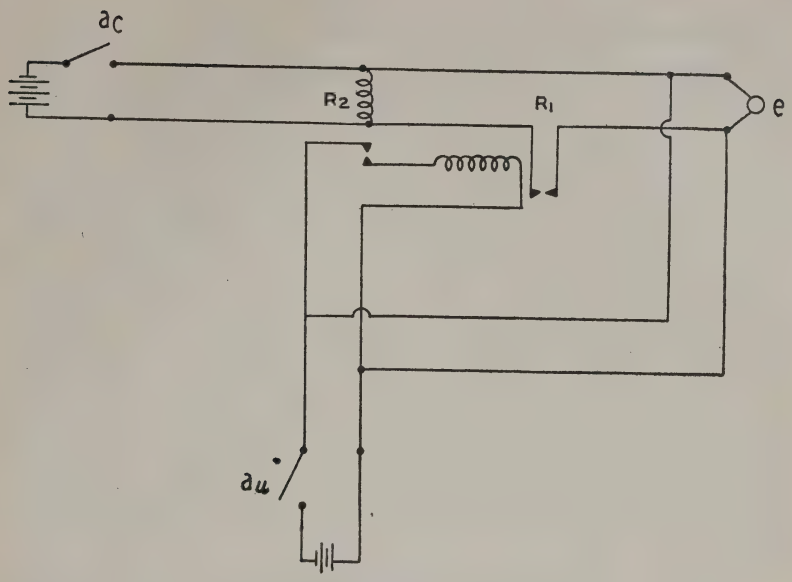

FIGURE 10

bination of elementary conditioned reflexes. We shall therefore consider first a mechanical model of this property.

A very ingenious model of that kind has been recently described by Hull and Baernstein (17). It will be very clear that this particular embodiment which they use is not the only one that is possible. The mercury toluene thermoregulators are used here as relays possessing a certain time-lag and inertia. Many other designs of

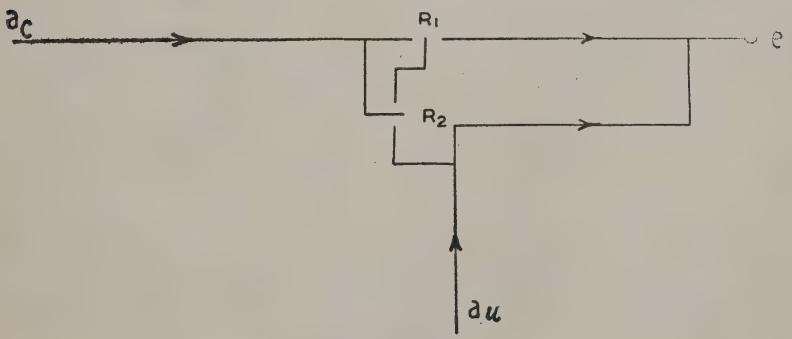

FIGURE 11

relays with such properties could be used. The essential is the general scheme of a connection of such relays which may be represented on Figure 10. e can be operated from the two circuits a_u and a_c. The former one sets e in operation in all cases, whereas in order to make e to be operated by a_c, the relay R_1 must be closed. But in order to close it, R_2 must be closed, too, and this requires the closing of a_c. If both relays are open, e can be operated by a_u only. If we close a_c this closes R_2. If after this we close a_u, this closes R_1. The relays are supposed to be of such a type that, once closed, they remain closed even after the current in their coils is interrupted. Under such conditions, the closing of a_c alone will now operate e. In the following considerations, where we shall use different combinations of such elementary conditioned reflex relays, we shall represent the scheme of Figure 10 in a simplified form, as in Figure 11. A device could be made without difficulty for the disconnection of the switch a_c whenever it is operated several times without being followed by the operation of a_u, and thus have a model of experimental extinction.[6]

It will be understood that each line represents the two wires of a complete electric circuit. We shall call such a connection an s-relay.

Now let us consider the scheme represented in Figure 12. Let e_1 be operated by a'_u unconditionally, and e_2 by a''_u. a'_c may be conditioned to either of them. e_1 and e_2 may represent two mechanisms of any kind, which act in opposite ways, so that their simultaneous operation gives the result zero. If we first condition e_1 to a'_c and then e_2 to a'_c, then the closing of a'_c will finally accomplish nothing. We may imagine e_1 to close the switch a''_u besides performing some kind of other function F. e_2 will destroy that function. Let all conditioned paths be open. Close first a'_c, then a'_u. This will condition a'_c to e_1 and perform F. But e_1 will also close a''_u, so that, when a'_c is applied several times, it becomes conditioned to e_2, which performs $-F$. We have here a simple case of short-circuiting. Let F symbolize the movement of the animal in a maze into a cul-de-sac. When it arrives, a stimulus is evoked which causes

[6]It must be remarked quite generally, that any of the properties possessed by multi-equilibria systems and illustrated by diagrams of the type of Figure 3 of (28) or of Figure 6 of this paper may be reproduced on a mechanical model, using a bead on a flexible wire, as suggested by Prof. K. Swartzel and discussed in the footnote of page 214 of (28). The bead may close, in different positions, different switches.

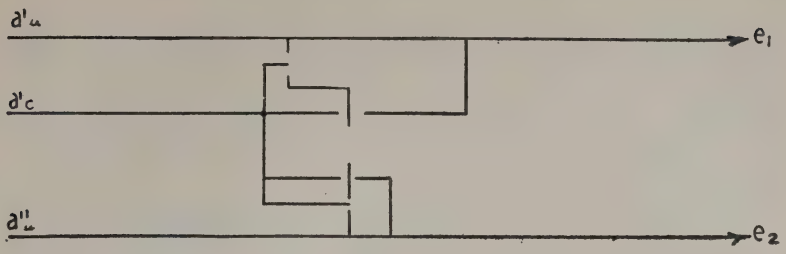

FIGURE 12

a reaction —F; that of coming back (e_1 — closes a''_u). The net result of the operation is that a'_c does not again cause the animal to go into that particular cul-de-sac. It may go into another if, like in Hull's sketch, a'_e is conditioned to several e's.

The next scheme will give us a model of a system which shows pattern-discrimination. In Figure 13 let P be a pipe filled with air, and let MF be a microphone-membrane, which, when vibrating, causes a pressure wave to travel along P in the direction shown by the arrow. At any moment the exact shape of the pressure wave

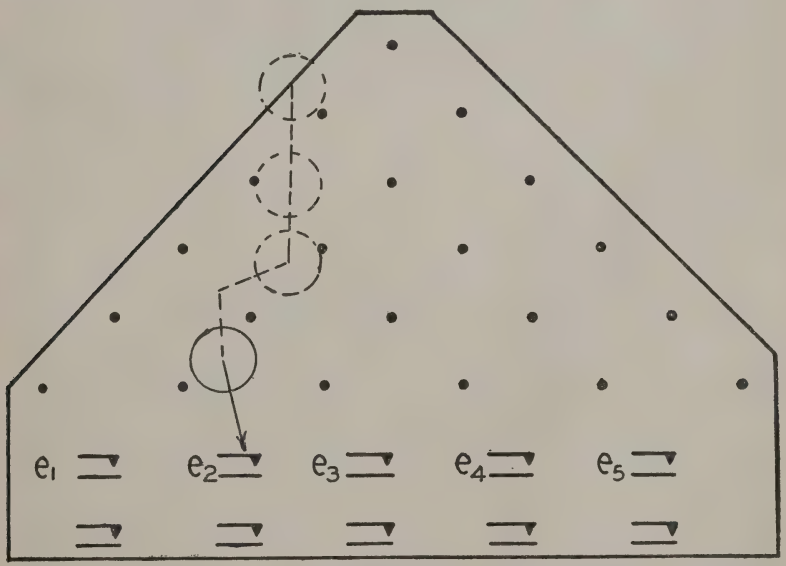

FIGURE 13

will depend on the mode of vibration of the membrane. If pressed in the middle and then released, it will give a different wave than if pressed at the edge, and so on. The different ways of setting the membrane into vibration represent the different stimulus-patterns. The different shapes of the pressure wave represent the different diffusion field of the *A*-substance.

Let a series of sensitive and practically inertialess manometers be arranged along *P*, and let these register the instantaneous pressure at each point. Let the dials of these manometers run along a series of contacts so that for each position of the dials, in other words, for each pressure at a given point, a different contact is closed. In Figure 13 for simplicity only three manometers M_1, M_2, and M_3 are shown. Let the operation of *MF*, in whatever way it may be done, close the circuit a_c which is connected through the manometer contacts and a series of *s*-relays to the bulb *e*, which can also be operated by a_u directly. Assume that at a time Δt after the pressure is exerted on *MF*, the pressure wave in *P* has such a shape that the manometer dials M_1, M_2, and M_3 occupy the positions 1, 3, 2, respectively. At that moment let a_u be closed. By closely following Figure 13, it will be seen that the *s*-relays s'_1, s''_3, and s'''_2 will close, all others remaining open. If we now "stimulate" *MF* exactly in the same way, but without reinforcing by a_u, we see that Δt seconds after the "stimulation" a closed circuit will be established between a_c and *e* via $1'$, s'_1, $3''$, s''_3, $2'''$, s'''_2, and *e* will flash. But, at any other time or for any other "stimulus pattern," *e* will not flash, because the pressure wave will have a different shape, and the manometer dials will connect the other circuits in which the corresponding *s*-relays are not closed.

Another scheme has been suggested to me by D. L. Watson. The motion of the membrane could be projected by means of a rotating mirror on a screen, so as to give different curves for different "stimulus patterns." The screen may be punched with several sets of holes behind which photocells connected with *s*-relays may be placed.

It now remains to give a model for the spontaneity of the action of an organism. We have seen that whether we assume that a certain stimulus evokes a set of reactions in an order determined by the previous history of the system, or otherwise, the distribution of these reactions will follow laws of chance. In the same way, the excitation of a d_i may actually be causally related to the total complex of

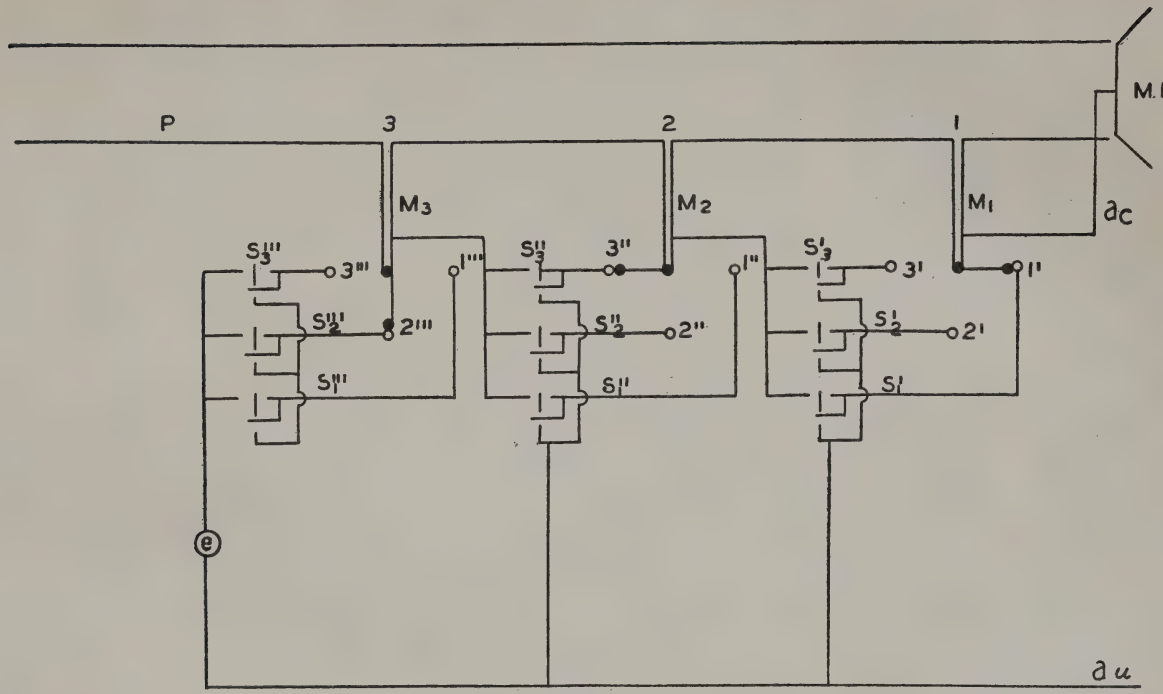

FIGURE 14

the surrounding, but for an external observer it comes by chance in a quite unpredictable way. We may symbolize this situation by using a Galton board (Figure 14) in which balls are thrown continuously by a mechanism driven by a motor. A set of switches is arranged along the lower edge of the board, and the ball falling on a switch will close it.

In this way the switches will be closed at random, so that there will be no way of predicting with certainty which switch is going to be closed next. On the other hand, the central switches will be closed more frequently, so that, depending on the arrangement of switches at the bottom of the board, the machine will show a tendency to close certain preferred switches.

Let us now return to Figure 8 and let the R's represent small lamps. They are all operated by corresponding switches at A, B, etc. Similarly, lamps are represented by the a-, e-, a'-parts. Furthermore, at each a'-part let us imagine a relay which short-circuits and thus discharges a battery, which is constantly slowly charged up and which operates a motor. This motor drives a mechanism which throws balls on a Galton board and thus operates various e-switches, which are connected to the corresponding R-lamps. Each e-switch can thus be operated either from outside or by the Galton board. Let all e-switches be arranged into two groups, so that to each switch e there corresponds an "antagonistic" switch, which is so connected to e that they can never be closed or opened simultaneously. (Various kinds of such switches are used in electrical engineering.) Let the e-switches at the bottom of the Galton board be arranged in such a way that below each one is located the corresponding "antagonistic" one, so that the ball in falling operates them in succession. A device can be made which will enable the ball, when the motor stops, to remain between the two antagonistic switches, keeping only the upper one closed, if the motor has just stopped, while the ball was between the two switches. Using several balls at a time, various e-switches may be closed simultaneously. Each a'-relay has its own battery and motor, though they may all operate the same Galton board. These batteries correspond to our d_i-mechanisms. The $a(d_i)$-parts will be made like the a-parts, except that they will be set in operation not by an "external" switch A, B, etc., but by the starting of a d_i-mechanism.

If a certain switch, say E, has not been operated for a sufficiently

long time, the corresponding d_E-battery will get sufficiently charged up, and start the motor and the Galton board.

All possible conditioned connections will be made by means of *s*-relays and their combinations. The connections between the $\bar{e}$-parts and the $\bar{a}_1$-parts can be made by means of relays which will close only when the current in the line e-$\bar{e}$ reaches or exceeds a certain value. In order to be able to regulate this current, we may make the *e*-switches of such a kind that they not merely close and open the current, but can vary it gradually from zero up. By connecting each $a(d)$-part with each *e*-part by a set of *s*-relays, connected in parallel and possessing each a different time-lag, we shall bring it about that the longer the $a(d)$ stimulus persists, the more *s*-relays will connect $a(d)$ with the corresponding *e*, and the stronger current the latter will send through e-$\bar{e}$.

As soon as the motor of the d_E-battery starts, the Galton board is set in operation, and we see that we can apply all the reasoning of Section X to our machine. The Galton board will continue "trying" until a'_E is closed. At that moment the ball stops, without closing the "antagonistic" switch, so that a (d_E) will become conditioned to the proper *e*-reaction, $(e + e')$ in the above case, all previous ones having been eliminated by "short-circuiting." The d_i-batteries and motors may be hidden inside a box, only the *A, B, C* switches and the *R*-lamps being outside. Thus an outside observer may not know with what "purpose" a certain act is done by the machine.

We wish at this point to remind the reader that Hans Driesch has written that he grants that a machine may be made which will speak German and even maintain a conversation in German; but not a machine which will learn German, and particularly tell occasionally an intentional lie (6, p. 83).

In view of the foregoing, we venture the opinion that, although an actual construction of a proper machine will be a matter of tremendous expense and labor, the above pessimistic statement is not sufficiently justified.

In conclusion, I wish to express my heartiest thanks to Dr. H. M. Johnson for a very thorough discussion of this paper and many very valuable comments and suggestions. My thanks are also due to Dr. Clark L. Hull and Dr. H. D. Baernstein. Dr. Hull has kindly permitted me to use his manuscript on "Knowledge and Purpose as Habit Mechanisms" (16) previous to its publication, and Dr. Baern-

stein lent me in the same way his manuscript on the reflex model (17).

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LES MÉCANISMES POSSIBLES DU CERVEAU ET LEURS MODÈLES PHYSIQUES

(Résumé)

Comme suite d'un memoir précédent une étude des systèmes à equilibres multiples est faite. Après avoir discuté quel genre particulier de ces systèmes est le plus probable du point de vue physiologique pour l'explication de divers phénomenès observés dans le cerveau et après avoir indiqué certaines experiences possibles pour decider cette question, une hypothèse est faite sur la structure physico-chimique et thermodynamique du neurone et des synapses. Ensuite il est demontré que cette hypothese explique nombre des faits, comme par exemple les differentes propriétés des reflexes conditionnés. Tels phénomènes comme l'apprentissage, le mensonge intentionné et le raisonnement logique reçoivent une interpretation physico-chimique à l'aide de cette hypothèse. De ce modèle du cerveau des consequences importantes doivent être tirées sur la nature de la logique qui doit être regardée comme l'expression de la structure physiologique du cerveau. Enfin il est demontré que même n'étant pas a present en état de préparer synthetiquement de tels systèmes physico-chimique, il est bien possible de construire des machines electro-mécaniques possedantes tous ces traits caracteristiques.

N. Rashevsky (N. Rachevsky)

MOGLICHE GEHIRNMECHANISME UND DEREN PHYSIKALISCHE MODELLE

(Referat)

In Anschluss an eine vorhergehende Abhandlung werden Systeme mit mehreren Gleichgewichten weiter besprochen. Es wird untersucht welche Art von solchen Systemen für die Erklärung der im Gehirn von höheren Tieren gefundenen Verhältnisse am wahrscheinlichsten in Frage kommt, und gewisse Versuche werden in diesem Zusammenhang vorgeschlagen. Auf Grund dieser Untersuchung wird eine bestimmte Annahme über die physikalisch-chemischen und thermodynamischen Eigenschaften der Neuronen und der Synapsen gemacht und es wird gezeigt dass diese Annahme eine Anzahl von Tatsachen, z.B. verschiedene Eigenschaften der bedingten Reflexe erklärt. Weiterhin werden auf Grund dieser Annahme solche Erscheinungen wie das Lernen, bewusstes Lügen und logisches Denken physikalisch-chemisch interpretiert. Es werden aus diesem Modell des Gehirns wichtige Schlüsse über die Natur der Logik gezogen. Letztere muss als Äusserung der physiologischen Konstitution des Gehirns aufgefasst werden. Endlich wird gezeigt, dass obwohl wir heute nicht imstande sind solche physikalisch-chemische Systeme künstlich herzustellen, es jedoch möglich ist elektromechanische Automaten zu bauen welche alle die erwehnten Eigenschaften besitzen.

N. Rashevsky (N. Raschewski)

SHORT ARTICLES AND NOTES

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CONSTRUCTING A TECHNICAL DICTIONARY*

HOWARD C. WARREN

The origin of the present attempt at a technical dictionary of psychology dates back fifteen years. At a meeting of the Association in December, 1915, on motion of Knight Dunlap, a committee of five was appointed "to consider the matter of uniformity of psychological terms." This committee labored assiduously for nine years, with a total output of 79 terms. Most of the work was done by mail and the effort to reach unanimous decisions entailed endless correspondence. This accounts for the meager results.

As chairman of the terminology committee my interest in the problem grew apace, and I came to entertain the idea of a much broader project—a lexicon covering the entire field of psychology. This would involve a systematic treatment of perhaps 8000, rather than 80 terms. It was evident that such an undertaking could not be carried out by a committee. A single responsible head was needed. There should be available a small group of experienced psychologists, to give advice on debatable points. But the ultimate power of decision must rest in the hands of the central head, to prevent undue waste of time.

The committee on terminology was discharged by the Association at the 1924 meeting, and the present speaker at once set to work on the larger project. The first step was to assemble as complete a list of psychological terms as possible. Various dictionaries were examined systematically, as well as the subject index of representative texts in all fields of psychology. All significant terms were checked off and copied in triplicate on card-catalogue slips. In this way a preliminary list of some 5500 terms was compiled. This concluded the first stage of the undertaking.

*A paper presented at the December, 1930, meeting of the American Psychological Association.

The second stage was to train the editor in the art of constructing definitions. This was no small task, for there was no teacher available. The only school was experience, and the only method of procedure was by trial and error.

There are several important desiderata in defining technical terms. The most essential point, of course, is accuracy. But scarcely less important is clearness of expression. A definition should be so worded that it will be readily understood, not merely by the technical expert in a given branch or topic, but by any one possessed of an elementary psychological training. It should not be ambiguously expressed; and it should not be tautological—it should convey some *new idea* to the reader.

Let me illustrate. A certain technical dictionary defines *sterility* as "inability to produce young." This is inaccurate; for some individuals may not be able to bring forth, though they may become impregnated. Again: A certain excellent statistical treatise defines an *array* as "the distribution of cases within a given column or row." This is accurate; but it does not enlighten the average reader; it would require considerable amplification before those lacking special training in statistical methods could form a clear idea of the meaning. Once more: In an Episcopal convention some years ago a learned prelate was asked to define the term *archdeacon*. He replied: "An archdeacon is an individual who exercises archidiaconal functions." This definition merely repeats the definiendum without adding any new thought.

Not only must the editor train himself to avoid these pitfalls, but he must determine the best form in which to present the material. How far should examples and discussion be included in the definition itself, and how far should they be treated as addenda? Should the pronunciation be indicated in all cases, or only where doubtful? Should cognate terms be grouped together, or defined each in its own alphabetic place. The arrangement finally adopted was determined largely by the psychological laws of attention and association. The definition should come immediately after the definiendum. Comment and discussion should follow the definition and should be clearly separated from it. Pronunciation and cognate forms might best be placed at the end.

My course of training was long and arduous. It consisted in attempting to define the entire list of terms which had been collected. I first took up various terms that interested me or that came up from time to time in my work or reading. When this random treatment exhausted itself, I started systematically through the alphabet from *a* to *zygote*. This part of the work occupied about four years. In going through the material alphabetically I encountered the terms already defined and revised them in the light of later experience. As the work proceeded, loose-leaf copies of the tentative definitions were typed in duplicate. One set was cut up and pasted on cards for expert treatment. The other set was bound up for editorial use. This completed the second stage.

The next step was to select a small Advisory Board and with their advice to choose the larger group of experts to prepare and revise the definitions. Practically everyone approached was willing to cooperate. Only one advisor declined out of nine selected. Out of 120 invited to collaborate as experts, 105 accepted; the 15 who declined gave excellent reasons. Before the collaborators were chosen it was necessary to map out the territory. In classifying topics and assigning experts to each, the advice of the Board was extremely useful. Without their counsel there would have been misfits among the collaborators.

In laying out the work, about 60 different topics or categories have been designated. Some topics cover large fields, such as comparative psychology and vision; some are quite narrow, such as eidetic phenomena, static sense, and graphic language. Certain experts have been selected to pass upon terms that carry special behavioristic or Gestalt or introspective meanings. Historical notes and logical distinctions among terms are listed as separate categories, and French and German equivalents are assigned to qualified experts. There are to be no biographical notices. Philosophy and theology are excluded, unless mind-body theories and folklore terms be considered such. As a rule, every definition must be passed upon by at least two independent collaborators. In case of irreconcilable differences, the question at issue will be put up to the Advisory Board, the Editor taking the final responsibility where the Board itself is divided.

The material at hand was sent out to the collaborators last September. Many of the lists have already been returned. The Editor is going over these, making annotations and passing them on to the second collaborator as rapidly as possible. Many additional terms have been suggested by various collaborators and assigned to them for definition. If the remaining collaborators are fairly prompt, this stage should be practically completed during the spring of 1931.

There will then remain (*a*) the historical notes, which will be confined to important traditional terms; (*b*) the treatment of antonyms and partial synonyms; (*c*) the specialized definitions required by various contemporary schools of psychology; and (*d*) the systematic preparation of French and German equivalents. It has been decided to give only those foreign terms that are substantially different in form from the English. An alphabetic index of foreign terms, with page references, will make these available for English readers and translators. As regards the schools of psychology, the aim will be to give separate definitions for each different usage, so far as necessary. But I have hopes that in the case of many subsidiary terms a single definition can be formulated, the difference of view being limited to the meaning of certain fundamental terms employed in these definitions. How far this is possible or desirable remains to be seen.

In closing, let me mention two practical difficulties which the Editor has had to face. Our clerical force is small; and it has been no easy task to

keep in personal touch with the numerous collaborators. A far more difficult problem was to formulate systematic rules for the collaborators. A set of instructions had to be prepared before the work started; they must provide for all the major contingencies foreseeable. Once these rules were printed and sent out, any change in procedure would entail useless labor or duplication of work on the part of the collaborators.

So far there has been little complaint or confusion or criticism. The only notable adverse comment has been directed by some of the collaborators against certain tentative definitions formulated by the Editor. I do not take this criticism to heart. For I realize full well that, despite my earnest endeavors, I have succeeded, alas, in attaining only a bowing acquaintance with certain members of our psychological vocabulary. But I *do* regret very much if my misconception of meanings has aroused any hostile emotion in the breasts of my fellow-workers. Be assured that we have no intention of tampering with established usage. I am far from approving the delightful lexicographical program sponsored by Will Rogers: "Give me a chance to write my own dictionary, and make a word mean anything I want it to."

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RANGE AND DISTRIBUTION OF TREMOR FREQUENCIES[1]

HAROLD RAY FOSSLER

In 1921, Hill (1) found that, in addition to the more common tremor frequency of 8-12 per second, a higher frequency of approximately 50 tremors per second could be shown, with the higher frequency superimposed upon the lower one. By means of the phonelescopic technique, Travis (2) has for several years been recording tremors of a frequency as high as 150 per second. In a recent paper by Travis and Hunter (3) a more refined technique, by which tremors of still higher frequency can be recorded, is described in detail.

The present study carries forward the investigation of tremor rates by this more refined technique on a larger number of cases and under a greater variety of conditions than has yet been done.

The apparatus, in brief, consists of a powerful electro-magnet energized by a current obtained from a 180-volt B-battery eliminator, and a small movable coil so supported by four narrow strips of thin dental rubber dam that it can move freely up and down in the intense field of the magnet.

[1]The writer wishes to acknowledge his indebtedness to Dean C. E. Seashore and to Prof. L. E. Travis for their permission to use the facilities of the laboratory.

The voltage induced in the coil activates a supersensitive element in a Westinghouse ocillograph. The recording is done on Eastman moving-picture film.

A normal group of 49 cases used in this study was made up of university students, the laboratory staff, and Psychopathic Hospital personnel. An abnormal group of 14 cases from the Psychopathic Hospital, sufficiently cooperative to participate readily in the experiment, was used for comparative purposes.

The subject assumed a standing position with the arm securely placed upon a firm support beneath the elbow. With the fingers vigorously extended, the ball of the middle finger was rested lightly upon the upper surface of the movable coil. Records were taken with first the right arm and then the left in this position.

The undulating wave formed by a series of tremors in the order of their occurrence shows that the lower frequency tremors determine the general contour of the wave. The tremors of higher frequency appear as secondary waves of less extent and amplitude on this general wave contour. These tremors of higher frequency may appear at any place on the lower frequency wave. Tremors of very high frequency and small amplitude appear as short volleys from 3-15 in number. The contour of such a wave of successive tremors shows considerable variation in form. On this basis three large divisions of cases could be made. In the first division, including 5% of the cases, the wave form is regular and even, since the low frequency tremors are quite similar in form and amplitude. In the second division, including 75% of the cases, the wave form is somewhat irregular, since the low frequency tremors present a greater variety in form and amplitude. In these cases, groups of similar low frequency tremors appear along with groups of dissimilar ones. In the third division, including 20% of the cases, the wave form is highly irregular, since the low frequency tremors are highly irregular in form and amplitude.

For each case studied, the actual number of tremors in each frequency group was expressed as a percentage of the total number for that case. The averages of these percentages for each group of cases under the different conditions appear in Table 1.

The frequency groups were made with due regard to the natural groupings found in the data.

When it is remembered that the higher frequencies are superimposed upon the lower ones, it is clear that the time occupied by any higher frequency is simultaneously occupied by the lower frequency upon which it is superimposed. A time comparison between the different frequencies is, therefore, extremely difficult to make, where several orders of frequency are involved. Thus, a small percentage value in Group 2, the basic low frequency group, does not mean a small proportion of time occupied by tremors of that frequency. On the contrary, the tremors in this group

TABLE 1

TABLE OF AVERAGE GROUP PERCENTAGES

Frequency group No.		1	2	3	4	5	6	7
Frequency per sec.		5-8	8-15	15-40	40-80	80-150	150-250	500
Normal	R*	4	7	46	18	13	1.5	.5
Group	L†	9	12	38.5	17.5	16	6.5	.5
	B‡	6.5	14.5	42.1	17.7	14.5	4	.5
Abnormal	R	6	11	30	17	36	0	
Group	L	6	10	22	20	39	3	
	B	6	10.5	26	18.5	37.5	1.5	
Finger	R	4	11	41	30	12	2	
Group	L	3	20	52	15	8	2	
	B	3.5	15.5	46.5	22.5	10	2	
Exercise Group	R	6	12	38	18	18	8	
Hot Bath Group	R		11	66	20	3		
Cold Bath Group	R	3	21	68	5	3		

*R=Right hand †L=Left hand ‡B=Both hands

generally occupy more time than those of any other single frequency. It is obvious, also, that any refinement in technique that permits the detection of the higher frequencies tends to raise the value of the average frequency. Hence, the averages shown above do not indicate that tremors with a frequency of 8-15 per second were found less frequently than by previous investigators. It simply indicates that proportionally a greater number of tremors of higher frequency were found.

Individual variability is an outstanding characteristic of most of the records. This is revealed by records from the same person from day to day as well as by records from different persons. A few individuals, however, present considerable constancy. Variability in the range and distribution of frequencies for the normal group is shown in terms of range groups.

Range (inclusive)	No. of cases
1-7	2
1-6	9
1-5	19
1-4	2
3-4	2
2-5	14
2-4	11
4-5	1
2-6	3

Similarly, the ranges covered by the two hands are shown from six cases selected at random.

Range for right hand (inclusive)	Range for left hand (inclusive)
4-5	1-6
2-3	1-5
2-4	1-5
2-5	1-7
2-3	1-5
3-5	1-4

Although there is considerable overlapping of ranges covered by the two hands, it is clear that each hand may show frequencies not appearing in the other.

A comparison of the records from the two hands reveal certain differences in the most common frequencies. The right hand shows a greater proportion of frequencies in Groups 2, 3, and 4. The range and distribution of frequencies are quite similar for both hands. It is quite probable that the high variability of the data detracts from the significance of such differences as appear.

It was thought that tremor rates might display some definite characteristics which could be used as a refined criterion of handedness, but such does not seem to be the case in this study.

In the endeavor to account in some measure for the high variability in the range and distribution of frequencies the size and complexity of the parts involved in the production of the tremors was considered. A smaller, less complex unit than the forearm should show a decrease in the lower frequency ranges and an increase in the higher ones if this factor of size and complexity is significant. Accordingly, records were taken as before from the ball of the middle finger but with the hand firmly supported on the proximal part of the palmar surface. The records, however, did not show the expected results. A greater proportion of frequencies is shown in the middle range, Groups 2-5 inclusive, but it is highly probable that this is not significant in view of the high variability shown in both groups. Lower frequencies appear here as often as before, and higher frequencies are not significantly increased.

Certain effects of exercise were likewise investigated. The subject was instructed to hold a hand-dynamometer at a pressure of 20 Kg. for one minute. Then a rest period of 10 seconds was allowed during which a record was taken with the arm supported at the elbow. The instrument was again held at the same pressure for another minute and another record taken during the following rest period. A third record was similarly taken at the end of the third minute. Here, again, the general range and distribution of frequencies are not strikingly different from the normal group. The same variability is shown in individual cases. In some instances the first exercise period is marked by an increase in the higher frequencies with a subsequent falling off in the later periods. In other cases lower frequencies are increased. Still other cases show no marked

differences in either direction. The results were so far from what was to be expected that further study of the exercise factor is being planned for the near future.

The cases studied were divided into four large groups on the basis of age. The first group included 28 cases from 19-25 years; the second group, 10 cases from 25-40 years; the third group, 7 cases from 40-65 years; and the fourth group, 4 cases from 60-76 years. This sampling showed no differentiating characteristics in one group not to be found in the others. The same general characteristics were found in all four groups.

A comparison was made between the normal group and a group of 14 cases, 7 men and 7 women, from the Psychopathic Hospital. Only such cases were used as would cooperate readily in the experiment. The results show some increase in the proportions of Groups 4 and 5, with a reduction in Group 3, the group having the largest proportion in normals. It is possible that a larger number of cases would have approximated very closely to the normal proportions. For this reason a detailed account of the neurological findings and diagnosis for each case is omitted.

No characteristic sex differences could be found.

Temperature effects were studied in the following way. A water bath at 48 degrees Centigrade was prepared, in which the right arm was immersed for 15 minutes. A record was then taken with the arm supported at the elbow. Similarly, a water bath at 8 degrees Centigrade was used. The general proportions in Group 3 show considerable increase above the normal, along with a decrease in the lower frequencies, by the hot bath. The general proportions in Group 3 remain high, lower frequencies appear normal, while higher frequencies are reduced, by the cold bath. These differences are as striking as any found in this study.

The writer hopes soon to follow with another study directed toward the discovery of the factors responsible for the high variability found in the records herein discussed. The exercise factor and its relation to pulse rate, breathing rate, blood pressure, and oxygen supply, as it appears in general bodily exercise, seem promising as approaches to the basic underlying physiological factors of tremor variability. Emotional states may do as much for the psychological factors.

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APPARATUS

AN AUDIO-FREQUENCY COMPOUND ALTERNATOR WITH INDEPENDENT CONTROL OF FREQUENCY, INTENSITY, AND PHASE

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The intricate relations of the problem concerned with the discrimination of phase changes in compound sounds to the history of auditory theory and the development of apparatus adequate to a solution of the problem have been outlined in another article by the writer (2). From this review were derived the specifications for controls indispensable to and sufficient for a decisive experimental test of the several conjunctive issues which remain in dispute. The present apparatus serves for only that aspect of the problem defined by the following considerations:

1. With a clang composed of two fundamentals, whose ratio terms for one commensurate period are represented by the symbols *H* and *L*, and of approximately equal intensity, the *two*[1] harmonic partials (*H*x*L*), and the differential partial (*H*-*L*) in combination, (*a*) what variations in clang quality occur when the phase of *L* is altered continuously relative to that of *H*? (*b*) which components of the clang can be discriminated as varying independently, and what are their relative magnitudes and periods? (*c*) what are the respective directions of these changes occurring simultaneously?

2. Maintaining a constant phase condition for the fundamentals *H* and *L*, and varying only the phase relations between two partials of identical frequency at the ratio level (*H*x*L*), are the same variations discriminated as in case (1)? This comparison would be differentiating between discriminations of *root mean square* and *instantaneous* pressure change conditions.

[1]It is apparent that the membrane of a telephone receiver, when acted on simultaneously by two forces whose periods of variation are in the ratio *H*:*L*, will have two sources of energy for the period (*H*x*L*); one being from the fundamental *H*, yielding its *L*'th harmonic; the other from the fundamental *L*, yielding its *H*'th harmonic. The total root mean square energy variation of the partial (*H*x*L*) will, then, depend upon the phase relation of *H* and *L*; however, only the instantaneous energy values of the compound due to *H* and *L* alone will vary as a consequent of a change in the phase relations of *H* and *L*, the *root mean square* value remaining *constant*. Any energy variation detectable by a *resonance analysis* must be of the *root mean square* order. It follows, then, that discrimination of *instantaneous* pressure variations not accompanied by *r.m.s.* variations is contrary to a resonance analysis function. This important consideration has been discussed and illustrated in another article (2).

The essential controls appertaining to the phase problem made available by the audio-phaser[2] apparatus are the following: (*a*) Two or more fundamental alternating currents are generated, having sinusoidal variation and unfiltered harmonic content less than 10 per cent of the intensity of the fundamental (25 to 30 decibels less).[3] (*b*) The frequency *ratios* of any combination of currents can be maintained *absolutely constant.* (*c*) The constancy of the frequency *level* of the fundamentals depends on the variations in the cyclic rate of the a.c. power used to drive the synchronous motor (Figure 1, *A*), and this usually varies about 0.5 to 1.1 cycles total in one half-hour. (*d*) Any frequency *level* between 20 and 5000 cycles can be obtained and held constant within the range of variations determinable from the error stated in (*c*). (*e*) Any desired frequency *ratio* within a double octave span (ratio 1:4) can be conveniently provided. (*f*) The power output[4] of this model of the audio-phaser can be adjusted, without attenuation networks in the circuit, between the levels 380 and 864 microwatts by adjusting the radial distance between the bi-polar inductors and rotors (Figure 1, *B* and *H*). (*g*) The sensation level of sounds produced with power from the audio-phaser actuating the coils of Baldwin mica-diaphragm, low impedance iron diaphragm telephone headsets, or the Western Electric 555-W moving coil type of receiver can be varied up to 60 or 70 decibels *without amplification.* (*h*) The phase relations between the fundamentals are continuously adjustable, so that all possible phase relations between the two currents can be attained in a relatively short but alterable period of time, without any interruption of the generating process, and without effective mistuning of the duad interval. (*i*) Any possible phase relation between the two fundamentals can be obtained and *held constant* as long as desired before a shift to another phase relation is effected. (*j*) By means of a special dial attached at *D,* Figure 1, the amount of phase change can be read off directly within 0.5-degrees accuracy. Each of these ten factors is indispensable to a solution of the problem, and in no previous experiment were *all ten* of these controls available.

The essential features of the audio-phaser can be observed in Figure 1. The output is directly from the bi-polar inductors (*B* and *C*). Although the coefficient of hysteresis for these poles was not computed, the loop ob-

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[2]Stewart (7) had a similar type of apparatus in his experiment on binaural phase. He used rotors having 2, 4, 6, 8, and 20 poles, on a common shaft driven at 6000 r.p.m.

[3]For crucial tests an insertion of two or more General Radio electric filters 330-A, 330-E, and 330-J reduces the low harmonic content of the audio-phaser output to sub-audible magnitude.

[4]If filters are not used in the external circuit the *percentage* of harmonic content in the audio-phaser output is less for the lower power level. A level of 400 microwatts is usually adequate for audition work with ordinary headphones.

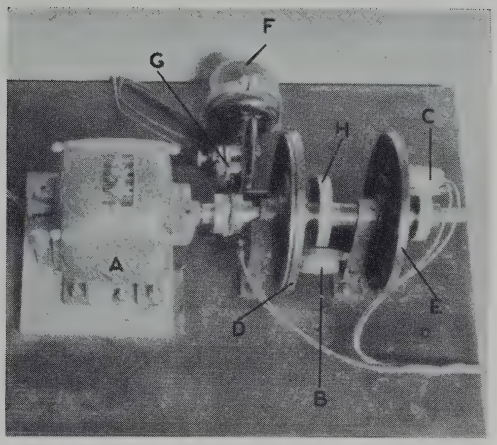

FIGURE 1

AN AUDIO-FREQUENCY COMPOUND ALTERNATOR PERMITTING INDEPENDENT CONTROL OF FREQUENCY LEVEL, FREQUENCY RATIO, INTENSITY, AND PHASE

served on a Braun tube oscillograph had very small area. The soft steel (*H*) used for the rotors also showed high permeability and low susceptibility.

The inductor units consist of two iron-core solenoids with rectangular faced poles within a permanent magnetic field. The two coils are connected in series and mounted in an aluminum case. The impedance of the inductor units varies from 272 ohms at 750 cycles to 596 ohms at 2400 cycles, the resistance values being 159 and 314 ohms, respectively. The mean potential drop across the output of this unit is 1.5 volts. Each inductor unit is attached by a rigid brass framework to bakelite discs (*D* and *E*); each of the latter rotates on a separate brass bushing having a mathematical center identical with that of the common hardened steel shaft on which the rotor units are mounted. Thus the path of the inductor poles is concentric with that of the rotor poles. A check on the perfection of this concentricity was made by measuring the maximum change in current output by rotating the inductor unit through 180° from the point of minimum current to the antipodal maximum. The ratio of change was found to be 0.18 decibels; consequently, irrelevant for hearing.

The pole faces of the rotor units are rectangular and convex, but are 0.028 inches wider than the faces of the inductor poles. The center-to-center distance of the inductor poles is equal to that of the rotor poles. The diameter of a rotor disc will vary as the number of poles on the periphery, since the space and pole widths are maintained the same for

each rotor. Each rotor is mounted on the common hardened steel shaft with a ring fit. Set screws are used also to further insure that the rotor units will not change their positions relative to each other during rotation. So long as this condition is maintained and the inductor discs (*D* and *E*) remain stationary, the frequency *ratio* of the inductor outputs remains *absolutely* constant.

During the rotation of the common shaft, a transient alternating current acts in the inductor coils. The cyclic rate of this current is the product of the rate of rotation of the common shaft and the number of poles on the rotor unit. The rotors mounted in Figure 1 have 48 and 32 poles, respectively, yielding a frequency ratio of 3:2 (the perfect fifth interval). The radial distances of the inductor poles from the common shaft center can be varied, permitting the exchange of rotors with different numbers of poles, and consequently the use of a variety of frequency ratios.

Various motors are used for driving the common shaft of the audio-phaser, depending upon whether a constant or variable frequency level is desired. The motor at *A*, Figure 1, is a General Electric 1/10 h.p., a.c. synchronous type, with a speed of 1800 r.p.m. When this motor is used, the outputs of the inductors have the frequencies 1440 and 960 cycles. The a.c. power supply usually varies not more than 1.8 per cent from 60 cycles, and the continuous oscillograph at the power house shows this maximum variation to be distributed over a period of twenty to thirty minutes. The variations of importance for the observations in this problem are not dependent on the maintenance of a constant frequency level for more than a few minutes. In cases where it is desired to vary the frequency level over the audio range, 250 to 4000 cycles approximately, a General Electric 1/8 h.p., d.c. compound-wound motor having a characteristic speed of 1725 r.p.m. is used for driving the audio-phaser shaft. Under constant load this motor runs sufficiently constant at speeds as low as 500 r.p.m.

The function of the phase-retarding unit (*F* and *G*, Figure 1) depends upon the following trigonometric considerations, from which we derive a specification of the conditions under which the phase effect is most likely to be discriminated, if it is of an audible order at all.

Consider the following equation for two sinusoidal variations in combination:

$$Y = \sin\ (Hx+e_1)+\sin\ (Lx+e_2) \qquad [1]$$

Here x is the time during which H cycles of one component and L cycles of the other component occur. e_1 and e_2 are the phases (in degrees) of each component at the origin (or Y-axis). Let $e_1=e_2 = 270°$, which will be noted hereafter as the phase of coincident minima, since in this condition both components pass through the origin at a phase of 270° (absolute minima).

It is clear that if we represent the *time* (abscissa value) of x in degrees, and equal to 360°, the second absolute minimum of the H-component will

be $360/L$ degrees in *advance* of the second absolute minimum of the L-component. Consequently, if the H-component is retarded by the amount $360/L$, the phase condition of coincident minima is obtained again. Thus Equations [2] and [3] are equivalent:

$$Y = \sin\,(Hx+270^\circ)+\sin\,(Lx+270^\circ) \qquad [2]$$

$$Y = \sin\left[\,Hx+\,(270^\circ-\frac{360^\circ}{L})\,\right]+\sin\,(Lx+270^\circ) \qquad [3]$$

If the H-component is retarded by half the amount $360^\circ/L$, or $180^\circ/L$, the maximum difference in compound wave *form* obtainable by a shift in phase between the fundamentals is obtained, and may be called the phase of coincident maxima. A retardation of the H-component by the amount $90^\circ/L$ gives the phase of coincident initial inflection points, and of $270^\circ/L$ that of coincident secondary inflection points. The phase of coincident initial inflection points is the ordinary "in phase" condition in alternating-current theory. Thus, three phase conditions between those of Equations [2] and [3] are represented by Equations [4], [5], and [6].

$$Y = \sin\left[\,Hx+\,(270^\circ-\frac{180^\circ}{L})\,\right]+\sin\,(Lx+270^\circ) \qquad [4]$$

$$Y = \sin\left[\,Hx+\,(270^\circ-\frac{90^\circ}{L})\,\right]+\sin\,(Lx+270^\circ) \qquad [5]$$

$$Y = \sin\left[\,Hx+\,(270^\circ-\frac{270^\circ}{L})\,\right]+\sin\,(Lx+270^\circ) \qquad [6]$$

If we number these phase conditions from 1 to 5 and use a parameter P to represent the phase condition desired, having a corresponding numerical value (1, 2, 3, 4, or 5), the generalized Equation [7] fits all cases.

$$Y = \sin\left[\,Hx+\,(270^\circ-\frac{90\,(P-1)}{L})\,\right]+\sin\,(Lx+270^\circ) \qquad [7]$$

The application of Equation [7] to a specific case of the frequency ratio 2/3, yields the following expressions for the five specified phase conditions:

1. $Y = \sin(3x+270^\circ)+\sin(2x+270^\circ)$
2. $Y = \sin(3x+225^\circ)+\sin(2x+270^\circ)$
3. $Y = \sin(3x+180^\circ)+\sin(2x+270^\circ)$
4. $Y = \sin(3x+135^\circ)+\sin(2x+270^\circ)$
5. $Y = \sin(3x+\ 90^\circ)+\sin(2x+270^\circ)$

During a shift from phase 1 to phase 5, all possible phase conditions are passed through. We shall denote this as one cyclic phase shift. The maximum differences in wave-form are obtained at phases 1, 3, and 5.

The amount of phase retardation in the H-component necessary for a

shift from phase 1 to phase 3 varies considerably for different frequency ratios. From Equation [7] one can readily compute this amount. The values for several important ratios follow: 1/3, 180°; 5/8, 36°; 9/10, 20°; 15/16, 12°.

The derivation of Equation [7] and its application to a variety of frequency ratios and frequency levels enable us to specify accurately the rate at which the phase-retarding disc must be rotated if the time during which a cyclic phase shift occurs is to be favorable to discrimination. This rate is also dependent on the number of poles (indirectly, the frequency level) of the *H*-component rotor. If we let T_r indicate the time duration (in *minutes*) for one rotation of the phase disc, t as the time in *seconds* for the duration of one cyclic phase shift, P_x as the amount in degrees of retardation in the *H*-component necessary to effect one cyclic phase shift, and N as the number of poles on the *H*-component rotor, then the relation follows:

$$T_r = \frac{6Nt}{P_x} \qquad [8]$$

If we substitute for P_x its corresponding value from Equation [7], that is, $\frac{360°}{L}$, then [8] becomes:

$$T_r = \frac{NLt}{60} \qquad [9]$$

The application of Equation [9] in relation to the phase-retarding unit (Figure 1, *G* and *F*) may be illustrated by selecting the ratios 1/3, 2/3, 1/2, and 5/8, used on this model of the audio-phaser. Assuming a value of three seconds for the time duration of one cyclic phase shift, the values for P_x and T_r for each of the four ratios selected are as in Table 1. Since

TABLE 1

Frequency ratio	No. rotor poles		P_x	T_r
	L	*H*		
1 : 3	24	72	360°	3.6
2 : 3	32	48	180°	4.8
1 : 2	32	64	360°	3.2
5 : 8	25	40	72°	10.0

values for T_r vary over such a wide range for different frequency ratios, even assuming a constant value for t, the phase-retarding device must be adjustable. Within certain limits the gear reduction unit (Figure 1, *G*) can be adjusted to different levels by exchanging gears, but a more suitable method is to use a variable-speed motor. From observations with the

audio-phaser we have found that the most favorable time value for the duration of one cyclic phase shift is about 3.5 to 4.0 seconds. The most satisfactory motor we have used is the General Electric SD-315, 1/50 h.p., 1725 r.p.m., d.c. motor having four leads out for series armature control. By the use of series resistance in series with the armature circuit, the speed of this motor can be reduced to as low as 600 r.p.m. with excellent regulation under constant load.

On the gear reducing device, at the point (Figure 1, *G*) a contact make-break is inserted. This contact is used to control a magnetically operated pen on a polygraph, making possible an objective recording of the rate, and instant of initiation and completion, of each cyclic phase shift. This technique is used in conjunction with parallel records made by observers tapping keys the instants at which any noticeable changes in tone quality occur during phase retardation. Lines across the record tape perpendicular to the direction of motion of the tape represent the same instant of time for each observer at points of intersection with individual pen lines. Thus the time relation between discriminations made by several subjects relative to each other and relative to the phase condition indicator can be directly compared.

In none of the former investigations were the considerations related to the derivation of Equations [7] and [9] and Table 1 utilized adequately in the development of apparatus controls. This may in part account for the disagreement between the results of the writer's experiments (1, 2) and those of previous investigators (3, 4, 5, 6). Unless the *cyclic* phase shift is recognized as being the only possible *stimulus-variant* resulting from *merely* a change in the *phase* between components of a compound sound, and unless this variation is made to occur within a time period favorable to discrimination, there is little expectancy that the phenomena can be demonstrated as of an audible order.

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BOOKS

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ADLER, ALFRED. *Problems of Neurosis.* New York: Cosmopolitan Book Corporation, 1930. Pp. v+244.

When, several years ago, an eminent litterateur made the complaint that no one could with profit turn to a modern psychological textbook and there find alleviation of his condition of soul sickness or discomfort, this book had not been written. While Adler's *Problems of Neurosis* is not a psychological text in the usual sense, it discloses a psychological system and thus subserves the primary *raison d'être* of most textbooks. That there are in it the possibilities for the mitigation of mental ills and the opportunities for relief and comfort no one who has examined it will deny. From cover to cover the book talks courage and forward-looking; by sheer weight of repetition it is likely to operate as a suggestive therapeutic. Since Adler has selected from his clinical experiences an enormous variety of case studies, the reader who approaches the volume with a personal problem is likely to find a description of his own ills. The hypochrondriac could gorge himself in these pages.

While the book is primarily intended to be a vehicle for the description of case histories, it is yet the occasion for the presenting of a systematic viewpoint in psychological theory. There is exposed no profound new principle, but the familiar tenets of individual psychology are amplified and illustrated. Further, competing modes of thought will find plenty at which to take offense, for the author is not entirely concerned with the positive statement of the contributions of his group. He takes many opportunities to marshal evidence designed to defeat the central concepts of his adversaries in psychoanalytic theory. In particular, he appears to derive considerable satisfaction (sexual?) from a display of the weaknesses of the notions of ambivalence, guilt complexes, castration complexes, and infantile sexual desires. The too frequent employment on the part of the

Freudians of the idea of sexual urges is ridiculed in more than one place. The broadening of the terms sadism and masochism is especially deplored.

Adler sees as the significant problem of life the adequate preparation for and maintenance of a nice adjustment to a threefold situation. Three problems must be solved by everyone; these are the questions of Society, Occupation, and Sex (Love, Marriage). Inadequacy in any of these relations results in neurosis. ". . . . Whoever can make friends with society, can pursue a useful occupation with faith and courage, and can adjust his sexual life in accordance with good social feeling, is immune from neurotic infection. But when an individual fails to square himself with one or more of these three inexorable demands of life, beware of feelings of abasement, beware of the consequent neurosis. Schizophrenia is the result of a failure in all three directions at once." The consideration of the case histories is patterned after this division of life problems.

The task of removing the neurotic symptoms is one fraught with difficulty, for the "style of life is founded in the first four or five years of childhood. This period closes with the full development of the ego and the consequent fixation of its attitude to life." As opposed to the Freudian tool of analysis and therapy, transference, Adler regards the function of the doctor who would properly treat neurosis to be that of a substitute mother. It is the mother, because she is the first, who interprets society to the individual and instils in him conceptions of work and play and brings about normal adjustments. When neurosis supervenes the maternal task devolves upon the doctor. "This is the twofold function of motherhood, to give the child the completest possible experience of human fellowship, and then to widen it into a life-attitude towards others." Good therapy, then, consists in gaining the patient's good will, recognizing his "life goal" and shaping his ideas to conform to it.

Perhaps the most engrossing of the chapters is that dealing with the early manifestations of social behavior as conditioned by chance family relationship. Adler has been frequently misunderstood in his stress on the matter of birth order. By devious processes he sets forth once more the doctrine that a child, regardless of the existence of older or younger brothers or sisters, may arrive at successful or unsuccessful adjustment to this vale of tears. Some of us, more casual observers, have long suspected this. Adler, by a nice ordering of case histories, demonstrates the possibilities in a clear manner. An answer to the problem of the "only child" is possibly one of the triumphs of "individual psychology."

His discussion of the subject of early recollections is not convincing. One feels that, for a psychology made up of whole cloth, Freud has done a more diverting thing in this connection. The time has fortunately arrived in psychology when experimental answers to such questions are possible—when plausible guesses have to give way to facts.

It is inevitable that precisely the case histories cited by Adler would pro-

vide equally useful material for interpretation by the methods of other psychoanalytic groups. One is startled at times that the same facts lend themselves so readily to interpretation and explanation in such widely contrasting terms. But this seems to be a characteristic of rational schemes. Perhaps, as in other scientific fields, we need more theories that can be disproved.

The preface to the volume, done by Dr. F. G. Crookshank, deserves a careful reading. Written from the point of view of an enthusiast and student of individual psychology, it has much to commend it. Not only does it very adequately outline the development of Adlerian psychology but it demonstrates many possibilities in the linkage of clinical psychology and practical medicine.

At some point in the preparation of the volume the editorial task has gone awry. Crookshank, in his prefatory essay, announces that "a bibliography compiled by Mr. Mairet [the editor] has been added" and lauds it for its usefulness. Unfortunately, the bibliography does not appear within the covers of the book.

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